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Abstract

This study examines the effect of the mandatory adoption of International Financial Reporting Standards (IFRS) on voluntary disclosure. Using a difference-in-difference analysis, we document a significant increase in the likelihood and frequency of management earnings forecasts following mandatory IFRS adoption, consistent with the notion that IFRS adoption alters firms' disclosure incentives in response to increased capital-market demand. We find the increase to be larger among firms domiciled in code-law countries, suggesting a catching-up effect among firms facing low disclosure incentives pre-adoption. We then propose and test three channels through which IFRS adoption could alter firms' disclosure incentives: improved earnings quality, increased shareholder demand, and increased analyst demand. We find evidence consistent with all three channels.

Keywords: Voluntary disclosure, IFRS, Management forecasts, Legal regime, Capital-market demand

I. INTRODUCTION

In 2005, more than two dozen countries around the world mandatorily changed their accounting standards to International Financial Reporting Standards (IFRS), and thousands of firms were simultaneously affected by this mandate. This event provides us with a plausible setting to examine whether the changes in mandatory financial reporting affect firms' voluntary disclosure behavior and, more importantly, to understand the underlying channels.

On the one hand, Ball, Robin, and Wu (2003) argue that accounting standards per se play a limited role in shaping firms' financial reporting practices. They posit instead that these practices are primarily determined by firms' disclosure incentives to meet the demand for transparency from outside capital markets. Therefore, if IFRS adoption is a mere change in accounting standards as a label, it should not have any impact on firms' voluntary disclosure behavior. On the other hand, a large body of literature documents various capital-market benefits associated with mandatory IFRS adoption (Daske, Hail, Leuz, and Verdi 2008; Li 2010; DeFond, Hu, Hung, and Li 2011; Tan, Wang, and Welker 2011; among others). Therefore, IFRS adoption could alter firms' disclosure incentives in response to the increased capital-market demand for disclosure.

To examine the average effect of IFRS adoption on voluntary disclosure, we employ a difference-in-difference research design to examine the changes in management earnings forecasts issued between 2002 and 2004 (pre-adoption) and between 2005 and 2010 (post-adoption). Our treatment sample includes mandatory adopters from 26 countries that mandated IFRS adoption in 2005. We use three different control samples: the first includes all non-IFRS adopters from 17 countries where IFRS was not mandated during our sample period; the second includes a propensity-score-matched (PSM) group of non-IFRS adopters from these non-IFRS-

mandating countries; the third control group includes firms from IFRS-mandating countries that voluntarily adopted IFRS prior to 2005. We document a significant increase in the likelihood and frequency of management forecast issuance following the mandatory IFRS adoption in our treatment sample for all three control samples after controlling for firm characteristics as well as country, year, and industry fixed effects. Greater increases are observed in countries whose pre-IFRS domestic standards differed more from the IFRS standards. We also examine whether the increase is due to IFRS adoption per se or concurrent changes in enforcement or reporting frequency. We do not find evidence supporting either alternative explanation.

Next, we explore cross-sectional variation in the effect of IFRS adoption on management forecasts. Ball, Kothari, and Robin (2000) argue that the demand for public disclosure is largely determined by different governance models used in common-law and code-law systems. Firms in common-law countries use a "shareholder" governance model and face high demand for public disclosure from outside capital markets. In contrast, firms in code-law countries use a "stakeholder" governance model and rely on private communications rather than public disclosure to resolve information asymmetry. Therefore, it is unclear whether the effects of IFRS adoption would apply to firms in code-law countries given the limited demand for disclosure from capital markets. However, compared to adopting countries' previous domestic standards, IFRS has a common-law origin and a capital-market focus. Given the various capital-market benefits, IFRS adoption may alter firms' disclosure incentives, even in code-law countries, and push their disclosure behavior toward a more transparent common-law regime. To examine the differential effect of IFRS adoption on firms with different governance models, we split our treatment sample into common-law and code-law countries. We observe a significant increase in management forecasts in both common-law and code-law countries following the adoption, with

the latter group experiencing even larger increases. Further analysis suggests a "catching-up effect" in forecasts among code-law firms, i.e., code-law firms issue fewer management forecasts than common-law firms pre-adoption, but they issue a similar amount of forecasts after IFRS adoption. These findings are consistent with the argument that IFRS adoption has driven code-law firms' disclosure incentives toward more voluntary disclosure.

We next examine the underlying channels for the increase in management forecasts among code-law firms. Based on prior literature, we propose and test three sources for the increase in capital-market demand for management forecasts: improved earnings quality, increased shareholder demand, and increased analyst demand. First, a large body of literature documents evidence suggesting improved accounting information quality following IFRS adoption (Barth, Landsman, and Lang 2008; Landsman, Maydew, and Thornock 2012; among others). As a result of the increased usefulness and informativeness of earnings, investors may demand more disclosure on future earnings. We use the level of discretionary accruals as an inverse measure for earnings quality. Second, recent literature finds that IFRS adoption attracts more investments from foreign institutions (Covrig, DeFond, and Hung 2007; DeFond et al. 2011; Florou and Pope 2012). Therefore, managers in IFRS-adopting countries may voluntarily disclose more to meet the higher demand for transparency from these sophisticated investors (Ajinkya, Bhojraj, and Sengupta 2005). We use foreign institutional ownership to measure shareholder demand. Third, prior literature documents increased analyst coverage following IFRS adoption (Byard, Li, and Yu 2011; Tan et al. 2011). Therefore, managers may issue more management forecasts to meet increased analyst demand (Lang and Lundholm 1996; Cotter, Tuna, and Wysocki 2006). We use the number of analysts following to measure analyst demand. We find that IFRS-adopting firms that experience larger increases in earnings quality,

shareholder demand, and analyst demand issue more management forecasts following IFRS adoption, especially among those domiciled in code-law countries. These findings corroborate our argument that IFRS adoption increases code-law firms' incentives for more disclosure.

Finally, we examine several alternative channels through which mandatory IFRS adoption could lead to more voluntary disclosure. The first two channels are the increased supply of information by the managers. First, firms may face higher litigation risk under "principles-based" IFRS standards and therefore disclose more to reduce the heightened risk. Second, managers may possess more forward-looking information under "fair-value oriented" IFRS standards and are therefore more able to supply such information. The third channel is the increased competition from peers. Under IFRS, firms across different countries report under a set of uniform accounting standards and face increased uniformity. Therefore, firms may disclose more as a result of increased peer pressure. The last alternative channel is the increased uncertainty about future earnings caused by a major standard change. Firms may try to disclose more to reduce such uncertainty. We do not find evidence supporting any of these alternative channels.

Our study makes several contributions. First, it adds to the literature that examines the effects of IFRS adoption. Prior studies generally document positive effects of IFRS adoption on firms' information environment and capital markets (Daske et al. 2008; Landsman et al. 2012; Byard et al. 2011; Tan et al. 2011; among others). Our study extends this stream of research by examining the effect of IFRS adoption on discretionary managerial disclosure behavior. The result that management forecasts increase following IFRS adoption suggests an additional mechanism through which IFRS adoption could improve firms' information environment and benefit capital markets. By documenting the differential effects in countries with different legal

origins and identifying the specific capital-market channels, our study complements Daske, Hail, Leuz, and Verdi (2013), which explores the heterogeneity in disclosure incentive changes as mechanisms through which voluntary IFRS adoption benefits capital markets.

Second, our paper adds to the literature on the interplay between mandatory financial reporting and voluntary disclosure. Prior studies in this area often rely on analyses within a single country and firm-level metrics for both mandatory and voluntary disclosure, which are endogenously determined (Francis, Nanda, and Olsson 2008; Ball, Jayaraman, and Shivakumar 2012). For example, managers of poorly governed firms may have incentives to maintain low levels of financial reporting quality as well as *choose* to provide little voluntary disclosure, which results in an observed positive association between mandatory reporting quality and voluntary disclosure. Without examining the specific mechanisms through which these two constructs are related, showing causality remains a challenge in these studies.[1] The IFRS setting allows us to explore the heterogeneity among IFRS-adopting countries and firms and to identify the changes in firms' disclosure incentives to meet capital-market demand as the underlying mechanisms driving the relation. Bischof and Daske (2013) find an increase in subsequent voluntary disclosure following a one-time mandatory disclosure of sovereign risk exposures by banks in the European Union (EU). They attribute this finding to a shift in voluntary disclosure equilibrium. Our paper complements their study by examining a more permanent and pervasive change in mandatory reporting and, more importantly, by testing the underlying channels through which mandatory reporting shifts the voluntary disclosure equilibrium.

[1] Although Ball et al. (2012) also study the effects of the introduction of SOX on voluntary disclosure, they acknowledge that the introduction is endogenous to the relatively unique events during that period, and shifts in litigation risks during that period could have caused the introduction of SOX and a simultaneous increase in voluntary disclosure. Although the introduction of IFRS could also be endogenous, the observed positive effects of the mandatory adoption on voluntary disclosure across a large number of countries are unlikely to be driven by a few countries' endogenous choice.

Third, our paper also contributes to the literature on management forecasts. Thus far, extant research in this area is mainly based on US data, and there is very limited evidence outside the US.[2] Our paper contributes to this stream of literature by providing large sample evidence on management forecasts across more than 40 countries. A concurrent paper by Ng, Tsang, and Yang (2012) examines a similar issue. Our paper differs from theirs in two ways. First, we document a catching-up effect in management forecasts between firms located in code-law and common-law countries. We argue that this effect is due to the common-law-oriented IFRS shifting firms' disclosure incentives toward a more capital-market-oriented regime. Second, we examine the specific channels through which mandatory IFRS adoption affects firms' disclosure incentives.

II. HYPOTHESIS DEVELOPMENT

A firm's voluntary disclosure decision is an equilibrium outcome of its underlying incentives and disincentives for disclosure. Ex ante, it is unclear whether and how a change in mandatory reporting standards, i.e., mandatory IFRS adoption, could have any substantial impact on a firm's voluntary disclosure behavior.

A mere change in accounting standards, without corresponding changes in firms' disclosure incentives, is unlikely to have any substantial impact on firms' disclosure behavior. Consistent with this argument, Ball et al. (2003) find that accounting standards per se play a limited role in shaping firms' financial reporting practices. Daske et al. (2013) find that IFRS has a limited capital-market impact among firms that only adopted them as a label without any

[2] Examples of studies examining management forecasts outside the US include Baginski, Hassell, and Kimbrough (2002), examining forecasts in Canada; Kato, Skinner, and Kunimura (2009), examining forecasts in Japan; Radhakrishnan, Tsang, and Yang (2012), examining forecasts around the world; and Huang, Li, Tse, and Tucker (2014), examining forecasts in China.

changes in their underlying disclosure incentives. Therefore, we should expect low or no impact of mandatory IFRS adoption on firms' voluntary disclosure behavior.

On the other hand, the objective of IFRS is to provide useful financial information to external investors in the decision-making process (IASB's Conceptual Framework for Financial Reporting). Consistent with this objective, there is a large body of literature documenting various capital-market benefits associated with mandatory IFRS adoption (Daske et al. 2008; Li 2010; DeFond et al. 2011; Tan et al. 2011; among others). We propose three channels through which IFRS adoption could alter firms' disclosure incentives in response to the increased capital-market demand.

First, firms may disclose more following mandatory IFRS adoption as a result of improved earnings quality. IFRS aims to produce high-quality financial statements, such as those reflecting economic substance more than legal form, those reflecting economic gains and losses in a more timely fashion, and those making earnings more informative (Ball 2006). Prior studies document both direct and indirect evidence consistent with this objective. For example, using a sample of voluntary adopters, Barth et al. (2008) find that firms applying IAS have higher accounting quality, as evidenced by less earnings management, more timely loss recognition, and more value-relevant accounting numbers. Landsman et al. (2012) find that the information content of earnings announcements increases following the mandatory adoption of IFRS. Wu and Zhang (2009) and Ozkan, Singer, and You (2012) find that accounting information is more useful for internal evaluation after IFRS adoption. Therefore, as accounting becomes more useful to investors for evaluation purposes, investors may demand more disclosure of future earnings to

help them make timely decisions.[3] Consistent with this argument, Francis et al. (2008) find that firms with higher earnings quality have more extensive voluntary disclosure.

Second, recent literature documents an increase in institutional ownership following IFRS adoption. For example, Covrig et al. (2007) and DeFond et al. (2011) find that both voluntary and mandatory IFRS adoption attracts more foreign mutual fund investors. Florou and Pope (2012) also document an increase in institutional holdings for mandatory IFRS adopters. Sophisticated investors, especially institutional shareholders, often have a higher demand for transparency and require more voluntary disclosure (Healy, Hutton, and Palepu 1999; Ajinkya et al. 2005). Therefore, firms may disclose more after IFRS adoption in response to the increased demand for transparency from these sophisticated shareholders.

Third, recent literature documents an increase in analyst coverage following IFRS adoption. For example, Tan et al. (2011) find that mandatory IFRS adoption attracts more analysts and improves the usefulness of accounting information to financial analysts. Landsman et al. (2012) and Daske et al. (2013) attribute the improved information content of earnings and capital-market benefits following IFRS adoption partially to higher analyst demand. As major users of firms' financial information, analysts often demand more public disclosure (Lang and Lundholm 1996; Cotter et al. 2006). Therefore, firms may disclose more after IFRS adoption in response to the increased demand from financial analysts.

Alternatively, mandatory IFRS adoption could also have a negative impact on voluntary disclosure. Jung and Kwon (1988) and Verrecchia (1990) show theoretically that the more is known about a set of risky assets a priori, the less pressure the market exerts on a manger to

[3] Another relevant paper along this line is Ball et al. (2012), who document a positive association between firms' audit fees and management forecasts. Ball et al. (2012) emphasize the verifiability, rather than the information quality, of mandatory financial reports. They argue that the desired quantities of audited financial reports and voluntary disclosure are jointly determined, and firms allocating more resources to voluntary disclosure are likely to allocate more resources to audited financial reports.

reveal what he or she knows privately. In other words, an exogenous decrease in the uncertainty about the value of assets due to an increase in earnings quality could reduce the need for voluntary disclosure. In addition, since institutional investors and analysts often possess their own means and resources to acquire private information, the need for public disclosure may diminish when firms' investor base becomes more sophisticated and when analyst coverage increases.[4]

Due to the above competing arguments, we propose the following two-tailed hypothesis:

H1: Mandatory IFRS adoption has no impact on voluntary disclosure.

Ball et al. (2000) argue that the demand for public disclosure is largely determined by different governance models used in common-law and code-law systems. In common-law countries, firms use a "shareholder" governance model. Shareholders alone elect the governing board. Because shareholders are the residual claimants, they have incentives to effectively monitor managers. Managers in common-law countries often face high monitoring pressures from external capital markets and analysts. In addition, because the ownership is often dispersed in common-law countries, outside shareholders face high information asymmetry and, therefore, greater demand for timely public disclosure. In contrast, firms in code-law countries rely on a "stakeholder" governance model, and various stakeholders, such as the government, lenders, and employees, often have private inside access to information. Thus, the demand for timely public disclosure is low.[5] Consistent with this argument, Ball et al. (2000) find that financial reporting quality, measured as timely loss recognition, is lower in code-law than common-law countries. Along the same line, Bushman, Piotroski, and Smith (2004) find that financial reports are less

[4] We thank the editor for suggesting this alternative view

[5] For example, in Germany, a code-law country, employees and banks both have representations on firms' supervisory boards. The supervisory board appoints and monitors the managerial board, which oversees financial reporting. Because managers often have close relations to these stakeholders, the information asymmetry is resolved through private communications.

transparent in countries with high state ownership, and Bharath, Sunder, and Sunder (2008) find that firms with better access to private credit face lower incentives for disclosure. Therefore, due to the low demand for public disclosure, we expect a low or no impact of mandatory IFRS adoption on firms domiciled in code-law countries where the demand for disclosure is low. On the other hand, compared with adopting-countries' prior domestic accounting standards, IFRS has a capital-market focus and a common-law orientation. Given the various capital-market benefits discussed above, IFRS adoption may alter code-law firms' disclosure incentives and push their disclosure behavior toward a more transparent common-law regime. Our second hypothesis with regard to a code-law regime is stated below:

H2: Mandatory IFRS adoption has no impact on voluntary disclosure in code-law countries.

III. DATA AND SAMPLE SELECTION

To proxy for voluntary disclosure, we focus on the extent to which managers provide earnings forecasts, the most prominent performance measure a firm supplies to investors. We obtain data on management earnings forecasts from Standard & Poor's Capital IQ database for the years between 2002 and 2010.[6] We begin with an initial sample of 42,922 firm-year observations from 30 countries that mandated IFRS in 2005. Next, we obtain accounting and stock price data from Compustat Global, institutional ownership data from FactSet, and analyst following data from I/B/E/S.[7] We remove 15,329 observations without sufficient data to

[6] A detailed description of the management forecast data in Capital IQ is included in the Internet Appendix.

[7] Firm-level information is drawn from Compustat Global rather than WorldScope to mitigate the data coverage concern. In our analysis, we treat firm-years without management forecasts as non-forecasters, but it could be the case that these firm-years were not covered by Capital IQ. Because Compustat and Capital IQ both belong to Standard & Poor's and because Capital IQ provides a data file directly linking Gvkey with Company ID (unique

calculate the control variables used in regressions. We exclude 464 firm-years in those countries that did not use IFRS post-adoption or did not use local accounting standards pre-adoption, or firm-years where the accounting standards were not disclosed.[8] We also exclude 6,837 observations for firms operating in financial industries (SIC 6000-6999), as they have different financial statements. This selection process generates a treatment sample of 20,292 firm-year observations from 26 countries.

We use three different control samples. The first includes all non-IFRS adopters from 17 countries where IFRS was not mandated during our sample period.[9] To construct this sample, we exclude firm-years if IFRS or IAS was voluntarily adopted during our sample period. We also exclude Chinese firms, as management forecasts were partially mandated in China.[10] We follow a similar approach to exclude firm-years without sufficient data to calculate the control variables used in regressions and financial firms. This approach generates a full control sample of 60,585 firm-year observations from 17 countries.

A concern with using all firms in non-IFRS countries as the control is their comparability with firms in IFRS countries in terms of disclosure incentives. To address this concern, we employ a propensity-score-matching (PSM) technique to pair firms in treatment and control groups based on observable characteristics. We first run a logit regression to model the

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company identifiers used in Compustat Global and Capital IQ, respectively), we believe Compustat Global's coverage is likely more similar to Capital IQ than WorldScope.

[8] Firms in our treatment countries that did not use IFRS after the mandatory adoption date might be those being exempted from mandatory adoption. For example, in certain countries, firms may be allowed to follow US GAAP for financial reporting. These observations are removed in our analysis to create a cleaner comparison between local GAAP in the pre-adoption period and IFRS in the post-adoption period.

[9] To maintain a clean control sample, we exclude countries that adopted IFRS in years other than 2005 during our sample period, including Singapore (2003), New Zealand (2007), Pakistan (2009), Israel (2008), Turkey (2008), and Brazil (2010).

[10] Chinese mandatory rules on management forecasts changed several times during our sample period, which contaminates China as a clean control group. See Huang et al. (2014) for a detailed discussion on this topic. Management forecasts in Japan were fully mandated during our sample period (Kato et al. 2009). We keep Japanese firms in our control group, as we expect mandatory IFRS adoption to have no impact on their forecast behavior. Our results are unchanged when we exclude Japan from our control group.

probability of a firm being in an IFRS country. We use all the firm-level controls used in our difference-in-difference analysis as well as industry and year fixed effects as determinants. We then match firms in IFRS countries with those in non-IFRS countries using the caliper technique (with replacement) with a radius of 0.01. This procedure is consistent with prior literature (DeFond, Hung, Li, and Li 2015). We use the matched group of non-IFRS adopters as the second control group. This approach generates a control sample of 20,292 firm-year observations.

The third control sample includes firms from IFRS-mandating countries that voluntarily adopted IFRS prior to 2005. Because these firms already reported under IFRS and experienced various capital-market benefits (Ashbaugh and Pincus 2001; Barth et al. 2008; Covrig et al. 2007), they should be less affected or unaffected by the mandatory adoption in 2005. Using firms from the same countries as controls could also mitigate the concern about different data coverage across IFRS and non-IFRS countries. We identify a firm as a voluntary adopter if it is located in an IFRS-mandating country and adopted IFRS or IAS before the mandatory adoption date. We exclude the years before a voluntary adopter adopted IFRS. This approach generates a control sample of 4,064 firm-year observations. As discussed in DeFond et al. (2015), each of these control samples has its advantages and disadvantages. Therefore, our conclusion is based on the collective results using all three control groups in our difference-in-difference analysis. To conserve space, we present our subsequent results using only the first control group, but we obtain robust results using the other two control groups.

Table 1 presents the sample distribution by country and by the calendar year of the forecast date. Panel A suggests that large economies, such as the UK, Australia, and France,

dominate the treatment sample, while the US and Japan dominate the control group.[11] This sample distribution is generally consistent with prior literature (Landsman et al. 2012; DeFond et al. 2015). Panel B indicates that our sample is roughly evenly distributed across the sample period. We also observe from this panel that the likelihood and frequency of management forecasts are much lower at the beginning of our sample period (2002-2003) for both IFRS and non-IFRS countries, likely due to smaller coverage of Capital IQ in early years. However, to the extent that the coverage issue exists for both the treatment and control groups, it should not affect our difference-in-difference results. Nevertheless, we conduct a series of robustness checks, including limiting our sample period to 2004-2010, restricting the analysis to firm-years with at least one forecast, and restricting the sample to firms that were forecasters in the pre-adoption period. We find consistent results throughout these checks (reported in Internet Appendix).

IV. PRIMARY EMPIRICAL RESEARCH DESIGN AND RESULTS

Average Effects of IFRS on Management Forecasts

To test H1, we employ a difference-in-difference method to evaluate the average effects of mandatory IFRS adoption on voluntary disclosure. Our basic research design entails estimating the following equation:

$$DISC = \beta_1 Post\_IFRS + Firm\ Controls + Country\ F.E. + Year\ F.E. + Industry\ F.E. \quad (1)$$

The dependent variable *DISC* represents a measure of voluntary disclosure. We use two variables, *Issue* and *Freq*, to measure the level of voluntary disclosure. *Issue* is a dummy variable defined as one if a firm issues at least one management forecast in a given year. *Freq* is a count variable

[11] Although Swiss firms were allowed to use US GAAP instead of IFRS during our sample period, we classify Switzerland as an IFRS-mandating country to be consistent with prior literature. However, our results are almost identical if we exclude the relatively small number of Swiss firms from our sample (unreported).

measuring the total number of management forecasts issued in a given year. We also separately count the number of new forecasts *Freq(New)*, forecast revisions *Freq(Revision)*, and forecast confirmations *(Confirm)*. We provide a detailed description and analysis for the different forecast types in the Internet Appendix. Both *Issue* and *Freq* are set to zero for firm-years without any management forecasts. We use a logit (Poisson) model when using *Issue* (*Freq*) as the dependent variable.[12] *Post_IFRS* is a dummy variable equal to one for firms in IFRS-mandating countries with fiscal years ending in or after December 2005. A positive (negative) β_1 indicates an average increase (decrease) in management forecasts after mandatory IFRS adoption. We cluster standard errors by country to correct for potential correlations among firms within the same country.

We control for various firm-level variables that could potentially influence firms' decisions for management forecasts, including *Size* (the natural logarithm of the market value of equity), *ROA* (net income divided by total assets), *BTM* (book value of equity divided by market value of equity), *Leverage* (long-term debt divided by total assets), *EarnVol* (standard deviation of earnings divided by total assets for the past five years, with a minimum of three years' data required), *RetVol* (standard deviation of annual buy-and-hold stock returns over the past five years, with a minimum of three years' data required), the number of analysts following (*Analysts*), and shares owned by foreign institutions as a percentage of total shares outstanding (*Inst*).

We expect larger firms and firms with greater financing needs to be more transparent. The association between management forecasts and the book-to-market ratio, earnings volatility, and stock return volatility is ambiguous. On the one hand, due to high uncertainty, managers of firms with higher growth rates or higher volatility may be endowed with less information and, therefore, issue fewer management forecasts. On the other hand, because high-growth and more

[12] Our conclusions remain unchanged if we use OLS and the natural logarithm of *1+Freq* as the dependent variable.

volatile firms often face higher information asymmetry, investors may demand more voluntary disclosure (Waymire 1985). Because firms often issue forecasts in response to analysts' and institutional investors' requests, we expect a positive association between management forecasts and analyst coverage and foreign institutional ownership (Lang and Lundholm 1996; Bhojraj, Blacconiere, and D'Souza 2004; Ajinkya et al. 2005). All the above firm controls are measured in the year preceding the forecast date. We also control for bad news (*BadNews*) by including a dummy variable defined as one if the firm experienced a negative change in earnings during the forecasting year, and zero otherwise. Firms may voluntarily disclose bad news early to avoid shareholder litigation (Skinner 1994), but they may also prefer to delay bad news until the earnings announcements (Roychowdhury and Sletten 2012). We also control for subsequent equity issuance (*EquityIssue*) by including a dummy variable defined as one if the number of common shares outstanding adjusted for stock splits and dividends increases by 20 percent or more in the subsequent year because firms may increase voluntary disclosure before accessing the equity market to reduce their cost of capital (Frankel, McNichols, and Wilson 1995; Francis et al. 2008). The *ADR* indicator is set to one if the firm has ADR traded in the US, and we expect ADR firms to face higher demand for disclosure from investors.

During our sample period, some of our IFRS-adopting countries increased their financial reporting frequency from semi-annual to quarterly reporting, which could have a positive impact on management forecast frequency, especially on the forecasts of interim earnings. Thus, we include a variable for reporting frequency (*Interim*) in the year of forecasting to control for this effect.[13] We convert all non-ratio variables into US dollars using the exchange rate at the corresponding fiscal year end. To mitigate the potential impact of outliers, we winsorize all continuous variables at 1 and 99 percentage levels. We also include fixed effects for country and

[13] We further address this issue in the robustness analysis section of the Internet Appendix.

year to control for unobserved country-specific and year-specific factors. We include industry fixed effects (2-digit SIC), as prior studies suggest that industry characteristics could affect firms' management forecast decisions (Li 2010; Ali, Klasa, and Yeung 2014).

Table 1, Panel A reports the average likelihood (*Issue*) and frequency of management forecasts (*Freq*) for each sample country. We observe large cross-country variation: the average forecast frequency ranges from 0.24 (South Africa) to 1.71 (Luxembourg) among IFRS countries, and it ranges from 0 (Peru) to 1.96 (United States) among non-IFRS countries. Panel B reports the average forecast likelihood and frequency by calendar year of the forecast date for IFRS and non-IFRS countries, respectively. We observe a large increase in forecasts in 2004 for both IFRS and non-IFRS countries. This could be a result of the increased popularity of management guidance around the world and/or the expanded coverage by Capital IQ, which underscores the importance of employing a difference-in-difference research design. We observe another increase in forecasts in 2005 and 2006 for IFRS countries, but not non-IFRS countries, consistent with the mandatory IFRS adoption in 2005 being associated with more management forecasts.

[Insert Table 1]

Table 2 reports summary statistics of regression variables in the pre- and post-IFRS adoption periods for all firms in IFRS and non-IFRS countries, along with differences in means between pre- and post-IFRS adoption. For firms in IFRS countries, 47 percent issued management forecasts during the post-adoption period relative to 14 percent during the pre-adoption period, and the average forecast frequency increased by 0.72. For firms in non-IFRS countries, we also observe an increase in forecast likelihood and frequency but with smaller magnitudes. The mean difference-in-difference values for *Issue* (*Freq*) is 0.27 (0.63), with a *t*-statistic of 37.13 (30.21), suggesting that firms in IFRS-adoption countries are 27 percent more

likely to issue management forecasts (issue 0.63 more management forecasts) post-adoption relative to firms in non-IFRS countries. These findings provide some preliminary evidence consistent with a positive effect of mandatory IFRS adoption on management forecasts. We also observe that firms in IFRS countries experience larger increases in size, leverage, return volatility, analyst coverage, foreign institutional ownership, and interim reporting frequency in the post-adoption period relative to the control group.

[Insert Table 2]

Table 3 presents the difference-in-difference regression results. Columns (1) and (2) report the results using the first control group, i.e., all firms in non-IFRS countries. The coefficients on *Post_IFRS* are positive and significant in both regressions. The coefficient of 1.550 (1.221) on *Post_IFRS* in the regression on *Issue* (*Freq*) suggests a marginal effect of 0.356 (1.149), i.e., firms in IFRS countries are 36 percent more likely to issue management forecasts (issue 1.15 more forecasts) in the post-adoption period than those in non-IFRS countries. The coefficients on the firm controls are generally consistent with prior literature. Positive coefficients on *Size*, *ROA, BTM,* and *Leverage* suggest that large, well-performing, mature, and highly levered firms issue more forecasts, while negative coefficients on *EarnVol* and *BadNews* suggest that firms facing high uncertainty and bad news are more reluctant to forecast. Positive coefficients on *Analysts* and *Inst* are consistent with management disclosing more to meet demand from analysts and shareholders. We also find that ADR firms and firms with higher interim reporting frequency issue more forecasts. We observe a negative coefficient on subsequent equity issuance, but it is only significant for the regression on *Issue*.[14]

Columns (3) and (4) report the regression results using the second control group, i.e., the propensity-score-matched firms in non-IFRS countries. Despite the smaller sample size, the

[14] The negative coefficient on subsequent equity issuance is also consistent with that in Francis et al. (2008).

coefficients on *Post_IFRS* continue to be positive and significant across both model specifications. Some control variables lose their significance, possibly due to the matching procedure in the first step.

Columns (5) and (6) report the regression results using the third control group, i.e., voluntary adopters in IFRS countries. Because this analysis only uses firms in IFRS countries, the sample size is further reduced. We replace the variable *Post_IFRS* in Equation (1) with a *Post_Mandatory* indicator defined as one for mandatory adopters with fiscal years ending in or after December 2005 and a firm-level *Mandatory* indicator defined as one for mandatory adopters and zero for voluntary adopters. We continue to find the coefficients on *Post_Mandatory* to be positive and significant across both model specifications, suggesting that relative to voluntary adopters within the same countries, mandatory adopters issue more management forecasts after the mandate. The negative coefficients on *Mandatory* suggest that voluntary adopters issue more management forecasts than local GAAP users during the pre-mandatory adoption period. Given that prior literature documents increased capital-market demand associated with voluntary IAS adoption (Ashbaugh and Pincus 2001; Barth et al. 2008; Covrig et al. 2007; Daske et al. 2013), this finding is also consistent with our argument that changes in firms' capital-market incentives following IFRS adoption lead to more voluntary disclosure. Interestingly, we find that the sum of coefficients on *Post_Mandatory* and *Mandatory* are negative and significant in both regressions, suggesting that mandatory IFRS adoption does not fully eliminate the disclosure gap between voluntary and mandatory adopters, perhaps because voluntary adopters still face relatively higher disclosure incentives than mandatory adopters even in post-adoption period. This is in line with the finding in Muller, Riedl, and

Sellhorn (2011) that differences in information asymmetry remain between mandatory and voluntary adopters even after all firms are required to adopt IFRS.[15]

We conduct a battery of robustness analyses addressing the concerns of changes in mandatory reporting frequency, and in sample composition that could potentially drive our results. We also conduct a country-level analysis. We obtain robust results across all these alternative specifications. To conserve space, we report and discuss these results in the Internet Appendix.

In summary, the results in this section suggest that both the likelihood and frequency of management forecasts increase significantly following mandatory IFRS adoption, i.e., an average positive effect of mandatory IFRS adoption on voluntary disclosure.

[Insert Table 3]

Legal Origin and IFRS Adoption Effects

To test H2, i.e. the differential effects of IFRS adoption on common-law and code-law countries, we re-estimate Equation (1), including all controls but allowing the coefficients on *Post_IFRS* to differ between common-law and code-law countries. The results using all firms in non-IFRS countries as the control group are reported in Table 4, Columns (1) and (2). We observe that both common-law and code-law IFRS countries issue more management forecasts (in terms of both forecasting likelihood and frequency) post-IFRS adoption relative to the control group. More importantly, the χ^2-test results suggest that the coefficient on *Post_IFRS* is significantly larger for the code-law group than the common-law group in the regression on forecast frequency. This finding suggests that voluntary disclosure increases after IFRS adoption, especially among code-law countries. In Columns (3) and (4), we test whether there is a "catching-up" effect in disclosure levels between code-law and common-law countries. To

[15] We thank one of the reviewers for providing this interpretation.

compare these disclosure levels during the pre-adoption period, we include two separate indicators, $IFRS_{Common}$ and $IFRS_{Code}$, and we omit country fixed effects from the regressions. The negative coefficients on both indicators suggest lower disclosure levels during the pre-adoption period for both common-law and code-law countries in the treatment group relative to the control group. We also find the coefficient on *IFRS* is marginally more negative for code-law countries than common-law countries, suggesting that the former group issued fewer management forecasts than the latter during the pre-adoption period. This is in line with the finding in Ball et al. (2000) that the financial reporting quality was lower in code-law countries during late 1980s and early 1990s, a period before IFRS was mandated. In addition, the larger coefficient on *Post_IFRS* for code-law countries and the similar coefficients on *IFRS+Post_IFRS* for code-law and common-law countries suggest a "catching-up effect" in disclosure levels among code-law countries, i.e., the differences in disclosure levels between these two groups were fully eliminated after IFRS adoption.

[Insert Table 4]

The above analysis uses a simple dichotomous classification based on legal origin. As discussed in Section II, the major reason firms face different disclosure incentives in common-law and code-law countries is their different governance models, i.e., shareholder vs. stakeholder. Therefore, we conduct a further analysis on the role of equity markets and other stakeholders, including government, labor unions, and lenders. We find some weak evidence suggesting that firms located in countries featuring high government involvement and small equity markets issue incrementally more management forecasts following IFRS adoption. These findings are again consistent with a catching-up effect among firms facing low capital-market incentives for

disclosure pre-adoption. To conserve space, we report and discuss these analyses and results in the Internet Appendix.

Channels for Changes in Disclosure Incentives

Our hypotheses predict that firms with greater improvements in earnings quality and increases in shareholder and analyst demand will experience larger increases in management forecasts following IFRS adoption, especially among code-law countries. To create a firm-year measure for earnings quality, we follow prior literature and use the cross-sectional modified Jones (1991) model.[16] We multiply discretionary accruals by -1 so that higher values indicate better earnings quality. We measure investor demand as the percentage of foreign institutional ownership, as prior studies find that foreign, rather than domestic, institutions appear to have higher demand for corporate governance (Ferreira and Matos 2008; Aggarwal, Erel, Ferreira, and Matos 2011).[17] In addition, both Covrig et al. (2007) and DeFond et al. (2011) find that IFRS adoption attracts more foreign mutual funds. We follow the approach in Ferreira and Matos (2008) and classify institutions into foreign and domestic and define foreign institutional ownership as the sum of the holdings of all institutions domiciled in a country different from the country where the stock is issued as a percentage of market capitalization. We measure analyst

[16] To calculate discretionary accruals for each firm-year, we estimate the following Jones (1991) model using firms from the same country, industry (2-digit SIC), and year: $\frac{Accruals_t}{Asset_{t-1}} = \alpha_1 \frac{1}{Asset_{t-1}} + \alpha_2 \frac{\Delta Sales_t}{Asset_{t-1}} + \alpha_3 \frac{PPE_t}{Asset_{t-1}}$. We require at least 20 observations to estimate the above equation. We then use the country-, industry-, and year-specific parameter estimates obtained from the equation to calculate the firm-year specific discretional accruals as a percentage of lagged total assets as $\frac{Accruals_t}{Asset_{t-1}} - (\bar{\alpha}_1 \frac{1}{Asset_{t-1}} + \bar{\alpha}_2 \frac{\Delta Sales_t - \Delta Receivables_t}{Asset_{t-1}} + \bar{\alpha}_3 \frac{PPE_t}{Asset_{t-1}})$, where $\bar{\alpha}_1$, $\bar{\alpha}_2$, and $\bar{\alpha}_3$are estimated coefficients for each country, industry, and year, respectively. This approach is similar to that in Pincus, Rajgobal, and Venkatachalam (2007), who use the scaled-decile rank of discretionary accruals estimated from the Jones (1991) model to measure earnings management. We obtain similar results if we use the absolute value of discretionary accruals or use the signed total accruals. Other international studies examining earnings quality use other measures, such as the correlation between accruals and cash flows and the variability of the changes in earnings (Barth et al. 2008). However, these measures can only be calculated at the aggregate level, not at the firm level.

[17] We obtain very similar results if we use total institutional ownership (unreported).

demand as the number of analysts following, as both Tan et al. (2011) and Byard et al. (2011) find that IFRS adoption attracts more analysts. These three channels capture different aspects of changes in firms' disclosure incentives.

The above three measures are calculated at the firm-year level. To construct the firm-level changes in disclosure incentives, we estimate the average of each measure for the post-adoption period (2005-2010) and the pre-adoption period (2002-2004) separately and then use the post- and pre-adoption difference to measure the firm-level changes. Using the firm-level average could reduce measurement errors and account for the possibility that these changes happened slowly over time. This step implicitly requires each firm to exist in both pre- and post-adoption periods.

We use the following equation to test the argument that changes in firm-level disclosure incentives after IFRS adoption lead to more management forecasts:

$$DISC = \beta_1 Post\_IFRS + \beta_2 Post\_IFRS \times \Delta Incentive + Firm\ Controls + Country\ F.E. + Year\ F.E. + Industry\ F.E. \quad (2)$$

where *ΔIncentive* is an indicator variable defined as one if the firm experiences above-median changes in disclosure incentives.[18] The sample median is calculated using changes in discretionary accruals, changes in foreign institutional ownership, or changes in analyst coverage among all treatment firms that exist in both pre- and post-adoption periods. Other variables are defined in the same way as in Equation (1). We expect a positive coefficient on β_2, which measures the incremental increase in management forecasts among firms experiencing a larger improvement in disclosure incentives within IFRS-adopting countries. To test the differential interaction effects of changes in disclosure incentives in common-law and code-law countries,

[18] The main effect of *ΔIncentive* is omitted because it cannot be measured for control firms, and we are interested in variation within the treatment group. Therefore, β_2 captures the incremental effect of IFRS adoption for firms with high disclosure incentives relative to those with low disclosure incentives within the treatment group.

we re-estimate Equation (2) by allowing the coefficients on *Post_IFRS* to differ between common-law and code-law countries. The regression results on forecast frequency are reported in Table 5. Columns (1) and (2) report the results using earnings quality to measure disclosure incentives.

We observe positive and significant coefficients on *Post_IFRS*×*ΔIncentive*, $Post\_IFRS_{Common}$×*ΔIncentive*, and $Post\_IFRS_{Code}$×*ΔIncentive*, suggesting that firms experiencing larger improvements in earnings quality issue more management forecasts after IFRS adoption in both common-law and code-law countries. This finding is consistent with the complementarity between mandatory and voluntary disclosure documented in the US setting (Francis et al. 2008). Columns (3) to (6) report the results using institutional ownership and analyst coverage as measures for disclosure incentives. We find the coefficient on *Post_IFRS*×*ΔIncentive* to be positive and significant only for code-law countries, consistent with the argument that code-law firms issue more management forecasts after IFRS adoption in response to higher demand from institutional investors and analysts. The finding that institutional investors and analysts do not seem to play an incremental role in encouraging management forecasts in common-law countries could be due to these firms already facing high capital-market demand during the pre-adoption period. When we put all three measures for disclosure incentives into one regression and run a horserace, we find similar results (unreported), suggesting that these three channels capture different aspects of changes in firm-level disclosure incentives.

[Insert Table 5]

In summary, the findings in this section suggest that mandatory IFRS adoption has increased firms' disclosure incentives through increased earnings quality and shareholder and analyst demand, leading to more management forecasts.

V. ADDITIONAL ANALYSES

In this section, we conduct additional analyses to explore 1) other potential channels through which IFRS adoption could affect management forecasts and 2) the cross-sectional variation of the adoption effect based on GAAP differences and enforcement strengths.

Other Potential Channels

Relative to prior domestic GAAP in many countries, IFRS is often referred to by accounting practitioners and academics as "principles-based", as it provides a broad set of principles that are subject to managerial interpretation and judgment. Therefore, firms may face higher litigation risk transitioning from rules-based to principles-based standards (Donelson, McInnis, and Mergenthaler 2012). In addition, "fair-value oriented" IFRS requires more timely recognition of fair-value asset losses and impairments. This could help outside investors know more precisely and in a timelier fashion what information managers possess, thereby increasing managers' litigation risk of withholding bad news.[19] Prior literature has identified litigation risk as an important determinant of voluntary disclosure, especially the disclosure of bad news (Skinner 1994). Therefore, we expect heightened litigation risk following mandatory IFRS adoption to lead to more voluntary disclosure. However, heighted litigation risk may also create obstacles to more voluntary disclosure, as concerns about potential lawsuits arising from inaccurate disclosure may discourage managers from issuing earnings forecasts. For example, Baginski et al. (2002) find that US firms issue fewer good-news and long-term forecasts than

[19] For instance, in a pre-IFRS regime, upon receiving a private signal of a significant decline in asset value, managers could withhold bad news and then profit from selling firm shares. They would face relatively low litigation risk, as investors would be unaware of the bad news until several quarters later when impairments (if any) are recorded. In an IFRS regime, where the decline in asset value is immediately reflected in financial statements as fair-value losses or impairments, managers who withhold bad news and sell shares in the same quarter would face high litigation risk.

Canadian firms due to fear of shareholder litigation. Therefore, whether IFRS adoption increases litigation risk, which in turn leads to more management forecasts, is an empirical question.[20]

We empirically assess heightened litigation risk as a potential channel for more voluntary disclosure. It is empirically difficult to measure the level of litigation risk anticipated by managers over time. However, we expect that firms operating in riskier industries are more likely to be affected by heightened litigation risk following IFRS adoption. Following prior literature, we identify risky industries as those with SIC codes within 2833-2936, 3570-3577, 7370-7374, 3600-3674, 5200-5961, or 8731-8734.[21] We test this prediction by replacing *ΔIncentive* in Equation (2) with an industry-level indicator variable defined as one for firms operating in a risky industry. We do not include the indicator variable itself in the regression, as it is subsumed by industry fixed effects. We continue to impose the requirement that each firm exists in both pre- and post-adoption periods to ensure that we have the same set of firms as those examined in Table 5. Table 6, Columns (1) and (2), report the results on forecast frequency. We observe a negative and significant coefficient on the interaction term for the whole sample as well as for both common-law and code-law countries. This finding suggests that firms in risky industries issue *incrementally fewer* management forecasts than those in other industries following IFRS adoption, inconsistent with the argument that heightened litigation risk under an IFRS regime leads to more voluntary disclosure.

The second potential channel for increased management forecasts following IFRS adoption is the increased supply of forward-looking information. Under "fair-value"-oriented

[20] The argument that IFRS increases litigation risk implicitly assumes that the disclosure requirements under IFRS are properly enforced and that managers are held accountable for withholding bad news. We discuss the role of enforcement later in this section.

[21] We acknowledge that this industry-based definition of litigation risk follows prior literature that mainly uses US firms as the setting. This could be a noisy measure for litigation risk in our setting of international firms, as these industries could face different regulatory and litigious environments outside the US. Nevertheless, the noise in the measure should work against us finding any significant results.

IFRS rules, managers are required to evaluate the current fair values of assets and liabilities, most of which are determined using forward-looking price/earnings multiples or other discounted cash flow models. Therefore, to comply with fair value rules under IFRS, managers have to collect forward-looking information and make projections about firms' future prospects, which reduces the marginal cost of producing forecasts for future earnings. Holding the disclosure incentive constant, given that managers already possess such information under an IFRS regime, they are more likely to disclose it. Because fair value accounting is more prevalent among financial firms that possess a large amount of financial assets and liabilities recognized under fair values, this argument predicts that financial firms should experience a larger increase in management forecasts than non-financial firms following IFRS adoption. Because our original sample excludes financial firms (SIC 6000-6999), we now add them back and examine whether IFRS adoption has a differential effect on financial and non-financial firms. We test this prediction by replacing *ΔIncentive* in Equation (2) with an industry-level indicator variable defined as one for firms operating in a financial industry.

The results reported in Table 6, Columns (3) and (4), suggest a similar increase in management forecasts for financial and non-financial firms after IFRS adoption, inconsistent with the argument that increased supply of forward-looking information leads to more voluntary disclosure.

Another potential explanation is increased peer pressure. After the mandatory adoption, firms in IFRS countries comply with a set of uniform accounting standards. The increased uniformity across countries may generate more peer pressure for disclosure. This argument predicts that firms facing a larger increase in peer pressure issue more forecasts following IFRS adoption. We use the industry uniformity measure developed in DeFond et al. (2011) to measure

peer pressure. Industry uniformity is defined as the number of firms within the same industry that use the same accounting standards. To calculate the change in uniformity, we average across the post-adoption period (2005-2010) and the pre-adoption period (2002-2004) and calculate the difference. We test this prediction by re-estimating Equation (2), where *ΔIncentive* is defined as one for firms operating in industries experiencing an above-median change in uniformity. Table 6, Columns (5) and (6), report the regression results. Inconsistent with the argument that increased industry peer pressure drives the increase in management forecasts, the coefficients on *Post_IFRS*× *ΔIncentive* are insignificant in both regressions.

[Insert Table 6]

Another potential explanation for more management forecasts following IFRS adoption is the higher uncertainty about future earnings during the transitional period immediately following the adoption. This may result in firms issuing more management forecasts to resolve this increase in uncertainty. This argument has two predictions: 1) the increase in management forecasts is larger among firms experiencing a larger increase in earnings volatility, and 2) the increase in management forecasts will be short-lived. To test the first prediction, we re-estimate Equation (2) by redefining *ΔIncentive* as one for firms experiencing above-median changes in earnings volatility in the post-adoption period. The results are reported in Table 6, Columns (7) and (8). Inconsistent with the explanation that the increase in management forecasts is caused by an increase in earnings volatility, we find the coefficient on *Post_IFRS*×*ΔIncentive* to be insignificant in both regressions.

To test the second prediction, we examine an alternative specification in which we allow the coefficient on *Post_IFRS* in Equation (1) to differ across different years after the adoption date. Under the alternative story, we expect only $Post\_IFRS_{2005}$ to be positive and significant.

The results as reported in Table 7, Columns (1) and (2), suggest that management forecasts start to increase immediately after the adoption in 2005 and increase further in the following years until 2010. This evidence is inconsistent with the story of a transitory effect. However, one may argue that management forecasts are sticky over time and that firms that provided forecasts before are more likely to do so again in the future. Therefore, even if IFRS adoption only has a transitory effect on management forecasts, the sticky disclosure behavior could make the effect seem non-transitory. To further examine this possibility, we analyze firms' tendency to *increase* forecast frequency.

The results are presented in Table 7, Columns (3) and (4). The dependent variable *Increase* is an indicator variable defined as one if forecast frequency increases from the previous year, and zero otherwise. The stickiness part of firms' forecasting behavior is therefore removed when we use *Increase* as the dependent variable. The positive and significant coefficients on *Post_IFRS* and the individual yearly indicators suggest that firms have higher tendency to increase forecast frequency after IFRS adoption, again inconsistent with a transitory effect.[22]

[Insert Table 7]

An alternative explanation for more management forecasts following IFRS adoption is the higher flexibility to manipulate earnings, which gives managers a better opportunity to smooth earnings and meet their own forecasts.[23] We provide two pieces of evidence inconsistent with this explanation. First, we observe more frequent forecast revisions post-IFRS adoption (reported in the Internet Appendix, Table IA2), inconsistent with the conjecture that earnings become easier to forecast under IFRS. Second, when we consider whether the increase in

[22] It is possible that firms affected by the financial crisis of 2008-2009 also provided more disclosure to reduce investor uncertainty.

[23] A few studies document increased earnings smoothing following mandatory IFRS adoption (Ahmed, Neel, and Wang 2013; Capkum, Collins, and Jeanjean 2013).

management forecasts is primarily driven by mandatory adopters with more opportunities to manipulate earnings, such as firms with larger abnormal accruals, we do not find any evidence in support of this claim. Instead, the results in Table 5, Columns (1) and (2), suggest that firms with greater increases in abnormal accruals (or smaller increases in earnings quality) in the post-adoption period actually issue fewer management forecasts.

Based on the analyses in this section, we fail to find evidence supporting the alternative channels, such as heightened litigation risk, higher supply of forward-looking information, increased peer pressure, higher uncertainty for future earnings, and increased earnings management, through which mandatory IFRS adoption leads to more management forecasts.

The Difference between IFRS and Prior Domestic GAAP

If the observed increase in management forecasts is caused by mandatory IFRS adoption, the increase should be positively associated with the degree to which IFRS alters a country's accounting standards. To test this implication, we re-estimate Equation (2) by replacing *ΔIncentive* with two country-level indexes: *BaeScore* and *BaeAcct*. Both indexes are constructed from item scores obtained from Bae, Tan, and Welker (2008, Table 1), who list 21 key accounting variables based on the Nobes (2001) *GAAP 2001 Survey* and assign a score of one for each item that does not conform to IAS. *BaeScore* is the sum of the 21 items (or variable *gaapdiff1* in Bae et al. 2008) and measures the aggregate difference between local GAAP and IFRS. *BaeAcct* is the sum of scores on items that directly affect financial statement numbers, as we are primarily interested in the extent to which IFRS alters firms' mandatory reporting numbers.[24] Table 1, Panel C, reports values for these two variables for our treatment countries. We conduct this analysis for IFRS-adopting countries only, and therefore, the variable

[24] See Ball, Li, and Shivakumar (2015) for a detailed definition of the *BaeAcct* index.

Post_IFRS is subsumed by fixed effects. Regression results are reported in Table 8, Columns (1) to (4). The positive coefficient on *Post_IFRS*×*Index* across all model specifications suggests that the increase in management forecasts in the post-adoption period is positively associated with the differences between prior domestic GAAP and IFRS.

[Insert Table 8]

Enforcement and IFRS Adoption Effects

Prior literature suggests that a mere adoption of different accounting standards does not appear to improve financial transparency unless combined with effective enforcement (Ball 2001; Ball et al. 2003). Consistent with this view, prior studies document an important role of legal enforcement in determining the effects of IFRS adoption on capital markets and information environment.[25] To examine the role of enforcement in our setting, we re-estimate Equation (2) by using the rule of law index (*Enforcement*) as the conditioning variable. The rule of law index for our sample countries measured in 2005 is obtained from Kaufmann, Kraay, and Mastruzzi (2009) and is reported in Table 1, Panel C. Table 8, Columns (5) and (6), report the regression results. Again, because this analysis is conducted for IFRS-adopting countries only, the variable *Post_IFRS* is subsumed by fixed effects. The coefficient on *Post_IFRS*×*Index* is insignificant in both models.

Christensen, Hail, and Leuz (2013) find that the equity market effects around IFRS adoption are confined to a small number of EU countries that concurrently enhanced enforcement. To evaluate whether our results are driven by concurrent enforcement changes, we conduct the following analyses: (1) we exclude EU countries with concurrent enforcement

[25] For example, Daske et al. (2008) find that the capital-market benefits of IFRS only exist in countries with strong enforcement; Byard et al. (2011) find that IFRS adoption only improves analysts' information environment in countries with both strong enforcement and domestic accounting standards that differ significantly from IFRS; and Landsman et al. (2012) find that the improvement in the information content of earnings after IFRS adoption is greater in countries with stronger legal enforcement.

changes from our treatment sample; (2) we re-estimate Equation (1) by allowing the coefficient on *Post_IFRS* to vary across EU and non-EU countries; and (3) we allow the coefficient on *Post_IFRS* to vary within EU countries based on whether the country had concurrent enforcement changes. Table 1, Panel C, reports the classification of IFRS countries into EU countries with and without concurrent enforcement changes. Table 8, Columns (7) to (12), report the results. We find that the coefficients on *Post_IFRS* continue to be positive and significant after excluding EU countries with concurrent enforcement changes. We also find the results to hold for both EU and non-EU countries, as well as for EU countries with and without concurrent enforcements. In particular, we find that the coefficients on *Post_IFRS* have similar magnitudes for EU and non-EU groups and even larger magnitudes for EU countries *without* concurrent enforcement changes. These findings suggest that our results are not restricted to the sample of EU countries or those with concurrent enforcement changes.

Based on the analysis in this section, we fail to find evidence supporting the argument that enforcement drives the increase in management forecasts after IFRS adoption.

VI. CONCLUSION

In this paper, we document a significant increase in the likelihood and frequency of management forecasts after mandatory IFRS adoption in 2005. We find the increase to be larger among code-law than common-law countries, suggesting a catching-up effect in voluntary disclosure after the adoption among countries that initially faced low demand for disclosure from capital markets. We propose and test three channels through which IFRS adoption could alter firms' disclosure incentives in response to increased capital-market demand: improved earnings quality, increased shareholder demand, and increased analyst demand. We find evidence

consistent with all three channels, especially among code-law countries. Finally, we find the effects to be positively associated with the difference between adopting-countries' prior local GAAP and IFRS but not with concurrent changes in enforcement.

We contribute to the literature by examining the effect of IFRS adoption on management forecasts, a previously unexplored area. We complement prior literature that examines the relation between mandatory reporting and voluntary disclosure within the US. Our finding that voluntary disclosure increases following improvements in mandatory reporting, especially in code-law countries, based on our analysis of the mandated IFRS adoption provides an economic rationale for an expectation that firms' disclosure incentives could change with changes in mandatory accounting standards.

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Table 1: Sample Composition and Institutional Variables

Panel A reports the number of firm-years and management forecast variables averaged by country. Panel B reports the number of firm-years and management forecast variables averaged by calendar year of forecast date. *Issue* is an indicator variable equal to one if a firm issued at least one management forecast in a certain year, and zero otherwise. *Freq* is the number of forecasts issued by a firm in a certain year. Panel C reports the legal origin, i.e., common-law versus code-law, and institutional variables for IFRS countries. The difference between local GAAP and IFRS (*BaeScore*) is based on Bae et al.'s (2008) summary score (*gaapdiff1*) of how domestic GAAP differs from IAS on 21 key accounting dimensions. *BaeAcct* is the sum of scores on items that directly affect financial statement numbers. Legal enforcement is based on the rule of law index (*Enforcement*) for the year 2005 from Kaufmann et al. (2009). Indicator variables designate European Union countries (*EU*), European Union countries with IFRS adoption bundled with substantive change in enforcement (*EU_ENF*), and European Union countries having no enforcement changes concurrent with IFRS adoption (*EU_nonENF*). These classifications are based on Christensen et al. (2013).

Panel A: Sample composition by country

Country	N	*Issue*	*Freq*	Country	N	*Issue*	*Freq*
IFRS countries				*Non-IFRS countries*			
Australia	2,590	0.41	0.79	Argentina	34	0.15	0.15
Austria	112	0.42	0.90	Canada	2,525	0.27	0.48
Belgium	386	0.35	0.68	Colombia	36	0.17	0.19
Czech Republic	19	0.16	0.63	Egypt	46	0.15	0.17
Denmark	597	0.56	1.61	India	1,308	0.22	0.32
Germany	1,174	0.57	1.47	Indonesia	687	0.27	0.38
Finland	761	0.56	1.33	Japan	23,431	0.48	1.31
France	2,142	0.36	0.72	Jordan	5	0.20	0.20
Greece	341	0.24	0.33	Korea	608	0.31	0.44
Hong Kong	480	0.26	0.34	Malaysia	2,934	0.16	0.20
Hungary	18	0.33	0.33	Mexico	195	0.26	0.48
Iceland	11	0.36	0.73	Peru	7	0.00	0.00
Ireland	217	0.42	0.99	Russia	107	0.42	0.83
Italy	1,029	0.37	0.65	Taiwan	739	0.25	0.39
Luxembourg	21	0.62	1.71	Thailand	1,388	0.40	0.70
Netherlands	627	0.47	0.86	United States	26,515	0.60	1.96
Norway	623	0.21	0.31	Zimbabwe	20	0.15	0.20
Philippines	90	0.26	0.66				
Poland	516	0.28	0.53				
Portugal	192	0.28	0.34				
South Africa	972	0.21	0.24				
Spain	551	0.34	0.50				
Sweden	990	0.24	0.40				
Switzerland	300	0.28	0.44				
United Kingdom	5,525	0.29	0.42				
Venezuela	8	0.25	0.25				

Panel B: Sample composition by year

Year	N	*Issue*	*Freq*	Year	N	*Issue*	*Freq*
	IFRS countries				*Non-IFRS countries*		
2002	1,940	0.011	0.014	2002	5,923	0.234	0.533
2003	2,113	0.013	0.017	2003	6,248	0.217	0.441
2004	2,247	0.259	0.357	2004	6,497	0.689	2.497
2005	2,197	0.365	0.602	2005	6,748	0.692	2.067
2006	1,809	0.473	0.857	2006	6,920	0.629	1.505
2007	2,042	0.458	0.851	2007	7,054	0.444	1.258
2008	2,299	0.436	0.873	2008	7,154	0.538	1.653
2009	2,783	0.447	0.876	2009	7,075	0.462	1.368
2010	2,862	0.564	1.203	2010	6,966	0.486	1.423
Total	20,292			Total	60,585		

Panel C: Institutional variables

Country	*Legal Regime*	*BaeScore*	*BaeAcct*	*Enforcement*	*EU*	*EU_ENF*	*EU_nonENF*
				IFRS countries			
Australia	Common	4	3	1.71	0	0	0
Austria	Code	12	6	1.86	1	0	1
Belgium	Code	13	8	1.24	1	0	1
Czech Republic	Code	14	9	0.86	1	0	1
Denmark	Code	11	7	1.95	1	0	1
Germany	Code	15	8	1.96	1	1	0
Finland	Code	12	8	1.40	1	1	0
France	Code	11	6	1.65	1	0	1
Greece	Code	17	10	0.77	1	0	1
Hong Kong	Common	3	2	1.60	0	0	0
Hungary	Code	13	7	0.83	1	0	1
Iceland	Code	N.A.	N.A.	1.98	1	1	0
Ireland	Common	1	1	1.57	1	0	1
Italy	Code	12	7	0.46	1	0	1
Luxembourg	Code	18	12	1.82	1	0	1
Netherlands	Code	4	3	1.75	1	1	0
Norway	Code	7	6	1.91	1	1	0
Philippines	Code	10	10	-0.37	0	0	0
Poland	Code	12	10	0.42	1	0	1
Portugal	Code	13	7	1.19	1	0	1
South Africa	Common	0	0	0.13	0	0	0
Spain	Code	16	9	1.10	1	0	1
Sweden	Code	10	7	1.78	1	0	1
Switzerland	Code	12	7	1.89	0	0	0
United Kingdom	Common	1	1	1.55	1	1	0
Venezuela	Code	5	3	-1.23	0	0	0

Table 2: Descriptive Statistics

This table reports summary statistics of regression variables separately for the pre-adoption (pre-2005) and post-adoption (post-2005) periods and for IFRS and non-IFRS countries, respectively. *Issue* is an indicator variable equal to one if a firm issued at least one management forecast in year *t*, and zero otherwise. *Freq* is the number of forecasts issued by a firm in year *t*. *Size* is the natural logarithm of market value of equity in year *t-1*. *ROA* is net income divided by total assets in year *t-1*. *BTM* is the book-to-market ratio in year *t-1*. *Leverage* is long-term debt divided by total assets in year *t-1*. *EarnVol* is the standard deviation of five annual earnings divided by total assets in year *t-1*. *RetVol* is the natural logarithm of the standard deviation of five annual stock returns in year *t*-1. *Analysts* is the number of analysts following in year *t-1*. *Inst* is the percentage of foreign institutional ownership in year *t-1*. *BadNews* is an indicator variable equal to one if the firm had a negative change in earnings in year *t*. *EquityIssue* is an indicator variable equal to one if the split-adjusted number of shares increased by 20 percent or more in year *t+1*. *ADR* is a firm-level indicator variable equal to one if a firm has ADR traded in the US. *Interim* is mandatory reporting frequency in year *t*, defined as one for annual reporting, two for semi-annual reporting, and four for quarterly reporting. This table also reports univariate difference-in-difference results. The "Difference" column compares mean values in pre- and post-adoption periods using a *t*-test. The "Diff-in-diff" column reports the mean difference-in-difference between IFRS countries and non-IFRS countries using a *t*-test.

	Pre-IFRS adoption				Post-IFRS adoption				Difference (Post-Pre)		Diff-in-diff (IFRS-Non-IFRS)	
	N	**Mean**	**Median**	**Std Dev**	**N**	**Mean**	**Median**	**Std Dev**	**Mean**	**t**	**Mean**	**t**
IFRS countries												
Issue	7,327	0.14	0.00	0.34	12,965	0.47	0.00	0.50	0.33	55.74	0.27	37.13
Freq	7,327	0.20	0.00	0.58	12,965	0.92	0.00	1.31	0.72	54.15	0.63	30.21
Size	7,327	5.02	4.88	2.12	12,965	5.43	5.30	2.14	0.41	13.14	0.20	5.51
ROA	7,327	0.08	0.10	0.22	12,965	0.09	0.10	0.18	0.01	3.06	0.00	0.58
BTM	7,327	1.20	0.67	12.19	12,965	1.04	0.57	8.71	-0.16	-1.02	-0.03	-0.16
Leverage	7,327	0.20	0.18	0.17	12,965	0.21	0.19	0.17	0.01	4.37	0.02	6.68
EarnVol	7,327	0.15	0.04	1.20	12,965	0.13	0.04	1.53	-0.02	-0.94	0.02	0.29
RetVol	7,327	-2.17	-2.20	0.54	12,965	-2.19	-2.20	0.55	-0.03	-3.20	0.05	5.23
Analysts	7,327	4.11	2.00	5.91	12,965	5.05	2.00	6.44	0.94	10.55	0.92	9.28
Inst	7,327	0.06	0.01	0.14	12,965	0.11	0.03	0.19	0.05	20.98	0.03	11.94
BadNews	7,327	0.34	0.00	0.47	12,965	0.42	0.00	0.49	0.08	11.54	0.01	0.85
EquityIssue	7,327	0.11	0.00	0.32	12,965	0.10	0.00	0.30	-0.01	-2.14	0.00	-0.72
ADR	7,327	0.08	0.00	0.27	12,965	0.07	0.00	0.26	-0.01	-2.00	-0.01	-3.39
Interim	7,327	2.78	2.00	1.02	12,965	3.97	4.00	0.27	1.19	98.31	0.99	67.27
Non-IFRS countries												
Issue	22,436	0.45	0.00	0.50	38,149	0.52	1.00	0.50	0.06	14.62		
Freq	22,436	1.37	0.00	1.89	38,149	1.47	1.00	1.91	0.09	5.93		
Size	22,436	5.37	5.22	2.05	38,149	5.59	5.47	2.03	0.21	12.39		
ROA	22,436	0.07	0.08	0.25	38,149	0.08	0.09	0.19	0.01	3.62		
BTM	22,436	1.02	0.75	0.88	38,149	0.88	0.66	0.78	-0.14	-19.41		
Leverage	22,436	0.21	0.19	0.19	38,149	0.21	0.18	0.18	-0.01	-5.58		
EarnVol	22,436	0.22	0.03	6.60	38,149	0.18	0.03	5.30	-0.03	-0.66		
RetVol	22,436	-2.16	-2.19	0.58	38,149	-2.24	-2.25	0.58	-0.07	-15.31		
Analysts	22,436	3.49	1.00	5.35	38,149	3.51	1.00	5.09	0.02	0.40		
Inst	22,436	0.03	0.00	0.08	38,149	0.04	0.01	0.10	0.02	26.26		
BadNews	22,436	0.37	0.00	0.48	38,149	0.45	0.00	0.50	0.07	18.10		
EquityIssue	22,436	0.07	0.00	0.26	38,149	0.06	0.00	0.25	-0.01	-2.86		
ADR	22,436	0.02	0.00	0.15	38,149	0.03	0.00	0.17	0.01	4.68		
Interim	22,436	3.06	4.00	1.01	38,149	3.27	4.00	0.97	0.20	24.08		

Table 3: Average Effects of IFRS on Management Forecasts

This table reports multivariate regression results for difference-in-difference analysis. Columns (1) and (2) report the results using all non-IFRS adopters as the control group. Columns (3) and (4) report the results using propensity-score-matched non-IFRS adopters as the control group. Columns (5) and (6) report the results using voluntary adopters from IFRS-adopting countries as the control group. *Post_IFRS* is defined as one for observations from the IFRS countries and with fiscal year ends on or after the mandatory adoption date, i.e., December 31, 2005, and zero otherwise. *Mandatory* is a firm-level indicator defined as one for mandatory adopters from the IFRS countries. *Post_Mandatory* is defined as one for mandatory adopters from the IFRS countries and with fiscal year ends on or after the mandatory adoption date, i.e., December 31, 2005, and zero otherwise. Other variables are defined in Table 2. We use a logit model for regressions on the binary variable, *Issue*, and a Poisson model for regressions on the count variable, *Freq*. We report coefficient estimates and standard errors (in parentheses) based on robust standard errors clustered by country. All regressions include country, year (calendar year of forecast date), and industry (2-digit SIC) fixed effects. All continuous variables are winsorized at the 1st and 99th percentiles. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

		Benchmark: All Non-IFRS Adopters		**Benchmark: PSM Non-IFRS Adopters**				**Benchmark: Voluntary Adopters**	
	Pred. Sign	(1) *Issue*	(2) *Freq*	(3) *Issue*	(4) *Freq*		Pred. Sign	(5) *Issue*	(6) *Freq*
Post_IFRS	+	**1.550****	**1.221*****	**1.131*****	**1.055*****	***Post_Mandatory***	+	**0.187*****	**0.166****
		(0.628)	**(0.332)**	**(0.329)**	**(0.241)**			**(0.050)**	**(0.074)**
						Mandatory	-	-0.267***	-0.406***
								(0.042)	(0.071)
Size	+	0.261***	0.141***	0.236***	0.161***	*Size*	+	0.165***	0.171***
		(0.036)	(0.007)	(0.039)	(0.019)			(0.014)	(0.020)
ROA	+	0.683***	0.550***	0.597***	0.362***	*ROA*	+	0.253*	0.549**
		(0.118)	(0.124)	(0.139)	(0.065)			(0.140)	(0.234)
BTM	?	0.003***	0.002***	0.001	0.001	*BTM*	?	-0.001	-0.001
		(0.001)	(0.000)	(0.002)	(0.001)			(0.002)	(0.002)
Leverage	+	0.288**	0.242***	0.098	0.138*	*Leverage*	+	0.220***	0.218
		(0.125)	(0.093)	(0.104)	(0.072)			(0.065)	(0.168)
EarnVol	?	-0.005*	-0.009***	-0.012	-0.022	*EarnVol*	?	-0.002	0.007
		(0.002)	(0.003)	(0.018)	(0.014)			(0.009)	(0.018)
RetVol	?	0.139*	0.041	0.149**	0.058	*RetVol*	?	0.112***	0.151***
		(0.084)	(0.041)	(0.072)	(0.040)			(0.031)	(0.044)
Analysts	+	0.027***	0.012**	0.022**	0.008	*Analysts*	+	0.008***	0.039***
		(0.004)	(0.006)	(0.009)	(0.007)			(0.003)	(0.010)
Inst	+	0.595**	0.307***	0.522***	0.235***	*Inst*	+	0.277***	0.614***

		(0.252)	(0.093)	(0.183)	(0.059)
BadNews	?	-0.148***	-0.086***	-0.140***	-0.091***
		(0.027)	(0.010)	(0.053)	(0.018)
EquityIssue	+	-0.052***	-0.028	-0.108***	-0.041*
		(0.016)	(0.025)	(0.040)	(0.024)
ADR	+	0.157	0.175**	0.156	0.203**
		(0.119)	(0.088)	(0.126)	(0.079)
Interim	+	0.338***	0.159**	0.356***	0.136**
		(0.106)	(0.070)	(0.123)	(0.059)
Year, Country, and Industry Fixed Effects Included					
Observations		80,877	80,877	40,584	40,584
Pseudo Rsq		0.220	0.242	0.226	0.276

		(0.080)	(0.191)
BadNews	?	-0.041*	-0.039
		(0.024)	(0.061)
EquityIssue	+	-0.014	-0.065*
		(0.032)	(0.035)
ADR	+	0.137***	0.198***
		(0.053)	(0.062)
Interim	+	0.097	0.072
		(0.080)	(0.088)
χ^2-test for Post_Mandatory+Mandatory=0 [p-value]			
Post_Mandatory+Mandatory		[0.00]	[0.01]
Year, Country, and Industry Fixed Effects Included			
Observations		24,356	24,356
Pseudo Rsq		0.274	0.264

Table 4: Analysis of Legal Regime

This table reports multivariate regression results for difference-in-difference analysis using all non-IFRS adopters as the control group. We split the treatment effect into countries with common-law and code-law origin. *Post_IFRS*$_{Common}$ (*Post_IFRS*$_{Code}$) is defined as one for observations from the IFRS countries with common-law (code-law) origin and with fiscal year ends on or after the mandatory adoption date, i.e., December 31, 2005, and zero otherwise. Other variables are defined in Table 2. We use a logit model for regressions on the binary variable, *Issue*, and a Poisson model for regressions on the count variable, *Freq*. We report coefficient estimates and standard errors (in parentheses) based on robust standard errors clustered by country. All regressions include firm-level controls (as defined in Table 3). All continuous variables are winsorized at the 1st and 99th percentiles. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

	Pred. Sign	(1) *Issue*	(2) *Freq*	(3) *Issue*	(4) *Freq*
Post_IFRS$_{Common}$	+	1.383*	1.019***	1.369**	0.944***
		(0.706)	(0.344)	(0.563)	(0.328)
Post_IFRS$_{Code}$	+	1.781***	1.521***	1.895***	1.665***
		(0.557)	(0.303)	(0.461)	(0.294)
IFRS$_{Common}$	-			-1.688***	-1.611***
				(0.510)	(0.364)
IFRS$_{Code}$	-			-2.216***	-2.110***
				(0.431)	(0.218)
			χ^2-test for difference [p-value]:		
Post_IFRS$_{Common}$= *Post_IFRS*$_{Code}$		[0.19]	[0.00]	[0.02]	[0.00]
IFRS$_{Common}$= *IFRS*$_{Code}$				[0.16]	[0.14]
IFRS$_{Common}$+*Post_IFRS*$_{Common}$= *IFRS*$_{Code}$+*Post_IFRS*$_{Code}$				[0.99]	[0.49]
Firm-level Controls		Yes	Yes	Yes	Yes
Year Fixed Effects		Yes	Yes	Yes	Yes
Industry Fixed Effects		Yes	Yes	Yes	Yes
Country Fixed Effects		Yes	Yes	No	No
Observations		80,877	80,877	80,877	80,877
Pseudo Rsq		0.220	0.242	0.174	0.189

Table 5: Analysis of Changes in Disclosure Incentive

This table reports multivariate regression results for difference-in-difference analysis using all non-IFRS adopters as the control group. We use changes in earnings quality, shareholder demand, and analyst demand as interaction variables. Earnings quality is defined as discretionary accruals using a modified Jones (1991) model multiplied by -1. Shareholder demand is defined as the percentage of foreign institutional ownership. Analyst demand is defined as the number of analysts following. Changes are calculated using the average between 2005 and 2010 minus the average between 2002 and 2004. Δ*Incentive* is defined as one for firms from IFRS countries with above-median changes in earnings quality, shareholder demand, or analyst demand, and zero otherwise. Other variables are defined in Table 4. We use a Poisson model for regressions on the count variable, *Freq*. We report coefficient estimates and standard errors (in parentheses) based on robust standard errors clustered by country. All regressions include firm-level controls (as defined in Table 3), country, year (calendar year of forecast date), and industry (2-digit SIC) fixed effects. All continuous variables are winsorized at the 1st and 99th percentiles. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

		Earnings Quality		Shareholder Demand		Analyst Demand	
	Pred. Sign	(1) *Freq*	(2) *Freq*	(3) *Freq*	(4) *Freq*	(5) *Freq*	(6) *Freq*
Post _IFRS	+	1.219***		1.197***		1.207***	
		(0.313)		(0.324)		(0.325)	
Post _IFRS* × Δ *Incentive	+	**0.114****		**0.077**		**0.066**	
		(0.046)		**(0.067)**		**(0.056)**	
$Post\_IFRS_{Common}$	+		1.034***		1.086***		1.100***
			(0.330)		(0.337)		(0.339)
$Post\_IFRS_{Code}$	+		1.466***		1.413***		1.391***
			(0.284)		(0.305)		(0.303)
$\boldsymbol{Post\_IFRS_{Common} \times \Delta\ Incentive}$	+		**0.092****		**-0.055**		**-0.075**
			(0.046)		(0.043)		(0.071)
$\boldsymbol{Post\_IFRS_{Code} \times \Delta\ Incentive}$	+		**0.164*****		**0.133***		**0.160****
			(0.052)		(0.072)		(0.076)
			Firm-level Controls Included				
			Year, Country, and Industry Fixed Effects Included				
Pseudo Rsq		0.253	0.253	0.253	0.253	0.253	0.253
Observations		68,537	68,537	68,537	68,537	68,537	68,537

Table 6: Analysis of Alternative Channels

This table reports multivariate regression results for difference-in-difference analysis using all non-IFRS adopters as the control group. In Columns (1) and (2), *ΔIncentive* is defined as one for firms in high litigation risk industries (i.e., with SIC being in 2833-2936, 3570-3577, 7370-7374, 3600-3674, 5200-5961, and 8731-8734), and zero otherwise. In Columns (3) to (4), *ΔIncentive* is defined as one for firms in financial industries (i.e., with SIC being in 6000-6999), and zero otherwise. In Columns (5) to (8), we use changes in peer pressure and earnings volatility as interaction variables. We use industry uniformity to measure peer pressure, defined as the number of firms within the same industry that use the same accounting standards. Changes in peer pressure are calculated using the average between 2005 and 2010 minus the average between 2002 and 2004. Changes in earnings volatility are calculated as the standard deviation of annual earnings divided by total assets for years between 2005 and 2010 minus the standard deviation of annual earnings divided by total assets for years between 2002 and 2004. *ΔIncentive* is defined as one for firms from IFRS countries with above-median changes in peer pressure or earnings volatility, and zero otherwise. We use a Poisson model for all regressions. We report coefficient estimates and standard errors (in parentheses) based on robust standard errors clustered by country. All regressions include firm-level controls (as defined in Table 3), country, year (calendar year of forecast date), and industry (2-digit SIC) fixed effects. All continuous variables are winsorized at the 1st and 99th percentiles. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

	Pred. Sign	Litigation Risk		Financial Industries		Peer Pressure		Earnings Volatility	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Freq	*Freq*	*Freq*	*Freq*	*Freq*	*Freq*	*Freq*	*Freq*
Post _IFRS	+	1.269***		1.314***		1.203***		1.240***	
		(0.312)		(0.331)		(0.311)		(0.318)	
Post _IFRS×ΔIncentive	+	-0.399***		0.148		0.066		0.005	
		(0.083)		(0.110)		(0.050)		(0.039)	
$Post\_IFRS_{Common}$	+		1.092***		1.119***		1.032***		1.020***
			(0.327)		(0.373)		(0.332)		(0.328)
$Post\_IFRS_{Code}$	+		1.507***		1.585***		1.540***		1.509***
			(0.284)		(0.321)		(0.257)		(0.286)
$Post\_IFRS_{Common}×ΔIncentive$	+		-0.404***		0.202		0.073		0.075
			(0.061)		(0.141)		(0.056)		(0.058)
$Post\_IFRS_{Code}×ΔIncentive$	+		-0.376**		-0.063		-0.057		-0.039
			(0.147)		(0.158)		(0.089)		(0.045)
				Firm-level Controls Included					
				Year, Country, and Industry Fixed Effects Included					
Pseudo Rsq		0.253	0.253	0.242	0.242	0.253	0.253	0.253	0.253
Observations		68,537	68,537	91,602	91,602	68,537	68,537	68,537	68,537

Table 7: Analysis of Persistence of Forecasts

In Columns (1) and (2), we split the treatment effect into different years. *Post_IFRS$_{20XX}$* is defined as one for observations from the IFRS countries, with fiscal year ends on or after the mandatory adoption date, and the forecast date in 20XX. In Columns (3) and (4), *Increase* is a dummy variable defined as one if forecast frequency increased from the prior period, and zero otherwise. Other variables are defined in Tables 2 and 4. We use a logit model for regressions on the binary variables, *Issue* and *Increase*, and a Poisson model for regressions on the count variable, *Freq*. We report coefficient estimates and standard errors (in parentheses) based on robust standard errors clustered by country. All regressions include firm-level controls (as defined in Table 3), country, year (calendar year of forecast date), and industry (2-digit SIC) fixed effects. All continuous variables are winsorized at the 1st and 99th percentiles. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

	Pred. Sign	(1) *Issue*	(2) *Freq*	(3) *Increase*	(4) *Increase*
Post _IFRS	+			1.327**	
				(0.569)	
Post _IFRS$_{2005}$	+	0.570***	0.793***		1.672**
		(0.170)	(0.083)		(0.839)
Post _IFRS$_{2006}$	+	1.365***	1.426***		2.151**
		(0.276)	(0.322)		(0.943)
Post _IFRS$_{2007}$	+	2.186***	1.577***		1.882**
		(0.812)	(0.474)		(0.811)
Post _IFRS$_{2008}$	+	1.686***	1.371***		1.123***
		(0.483)	(0.252)		(0.282)
Post _IFRS$_{2009}$	+	2.038**	1.524***		1.964*
		(0.919)	(0.424)		(1.011)
Post _IFRS$_{2010}$	+	2.410**	1.748***		2.040***
		(0.996)	(0.412)		(0.768)
		Firm-level Controls Included			
		Year, Country, and Industry Fixed Effects Included			
Pseudo Rsq		0.226	0.244	0.105	0.111
Observations		80,877	80,877	80,877	80,877

Table 8: Analysis of GAAP Distance and Enforcement

In Columns (1) to (6), only firms from IFRS countries are used, and *Index* is defined as a *BaeScore*, *BaeAcct*, or *Enforcement* index (as defined in Table 1). Columns (7) to (12) report regression results for difference-in-difference analysis using all non-IFRS adopters as the control group. In Columns (7) and (8), we exclude EU countries with bundled enforcement changes, i.e., *EU_ENF*=1. In Columns (9) to (12), we split the treatment effect into countries within and outside the EU. The former is further split into those with and without bundled enforcement changes, as classified by Christensen et al. (2013). $Post\_IFRS_{EU\_ENF}$ is defined as one for observations from the EU with fiscal year ends on or after the mandatory adoption date, and zero otherwise. $Post\_IFRS_{EU\_ENF}$ is defined as one for observations from EU countries with bundled enforcement changes and fiscal year ends on or after the mandatory adoption date, and zero otherwise. $Post\_IFRS_{EU\_nonENF}$ is defined as one for observations from EU countries that do not have bundled enforcement changes with fiscal year ends on or after the mandatory adoption date, and zero otherwise. $Post\_IFRS_{non\text{-}EU}$ is defined as one for observations from the IFRS countries but outside the EU (*EU*=0) with fiscal year ends on or after the mandatory adoption date, and zero otherwise. We also report *p*-values of χ^2-tests by comparing the coefficients of $Post\_IFRS_{EU}$ with $Post\_IFRS_{non\text{-}EU}$ and $Post\_IFRS_{EU\_ENF}$ with $Post\_IFRS_{EU\_nonENF}$. See Table 1, Panel C, for definitions of *BaeScore*, *BaeAcct*, *Enforcement*, *EU*, *EU_ENF*, and *EU_nonENF*. Other variables are defined in Table 3. We use a logit model for regressions on the binary variable, *Issue*, and a Poisson model for regressions on the count variable, *Freq*. We report coefficient estimates and standard errors (in parentheses) based on robust standard errors clustered by country. All regressions include firm-level controls (as defined in Table 3), country, year (calendar year of forecast date), and industry (2-digit SIC) fixed effects. All continuous variables are winsorized at the 1st and 99th percentiles. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

		BaeScore		*BaeAcct*		*Enforcement*	
	Pred. Sign	(1) *Freq*	(2) *Issue*	(3) *Freq*	(4) *Issue*	(5) *Freq*	(6) *Issue*
Post_IFRS×Index	+	0.030**	0.033*	0.046**	0.052*	0.055	0.121
		(0.013)	(0.019)	(0.022)	(0.031)	(0.069)	(0.116)
Observations		20,281	20,281	20,281	20,281	20,292	20,292
Pseudo Rsq		0.266	0.249	0.266	0.250	0.265	0.249

		Ex. EU countries with Bundled Enforcement Changes		EU vs. Non-EU		EU with vs. without Bundled Enforcement Changes	
	Pred. Sign	(7) *Freq*	(8) *Issue*	(9) *Freq*	(10) *Issue*	(11) *Freq*	(12) *Issue*
Post_IFRS	+	1.291***	1.726**				
		(0.358)	(0.727)				
$Post\_IFRS_{EU}$	+			1.308***	1.520***		
				(0.330)	(0.574)		
$Post\_IFRS_{EU\ ENF}$	+					1.147***	1.335**
						(0.343)	(0.576)
$Post\_IFRS_{EU\ nonENF}$	+					1.513***	1.773***
						(0.304)	(0.565)
$Post\_IFRS_{non\text{-}EU}$	+			1.082***	1.617**	1.080***	1.608**
				(0.367)	(0.788)	(0.368)	(0.788)
		χ^2-test for difference [p-value]:					
$Post\_IFRS_{EU}$= $Post\_IFRS_{non\text{-}EU}$				[0.23]	[0.79]		
$Post\_IFRS_{EU\_ENF}$= $Post\_IFRS_{EU\ nonENF}$						[0.08]	[0.04]
Observations		72,156	72,156	80,877	80,877	80,877	80,877
Pseudo Rsq		0.234	0.220	0.242	0.220	0.242	0.220

Internet Appendix

to

"Mandatory Financial Reporting and Voluntary Disclosure: The Effect of Mandatory IFRS Adoption on Management Forecasts"

Internet Appendix I: Capital IQ Data and Forecast Type

Capital IQ started collecting information on corporate guidance in text format from January 2002 for firms across 90 countries using various public sources, including press releases and articles from more than 20,000 news wires and publications, regulatory files, company websites, web agents, conference call transcripts, and investor conference organizer websites. If the information is in local languages, Capital IQ translates the manuscript into English. Capital IQ also provides information on company identifiers, forecast headlines, news sources, and forecasting dates in a machine-readable format, which allows for easy merge with other databases. We extract texts of management earnings guidance from Capital IQ Key Development by performing a search for keywords, including "Earning", "earning", and "EPS", in headlines and main texts under the Key Development Type "Corporate Guidance". We download all output guidance issued between 2002 and 2010. To remove possible duplications, if multiple news articles are issued on the same date for the same firm, we keep only one observation in our sample. A summary of the initial sample of forecasts downloaded from Capital IQ is provided in Table IA1. We also tabulate the number of forecasts issued between 2004 and 2009, the sample period used in Radhakrishnan, Tsang, and Yang (2012) and find a similar sample composition.[1]

Capital IQ also classifies Key Development Types into either "New/Confirmed" or "Raised/Lowered". To separate new forecasts from forecast confirmations, we use information from Key Development Headline. A forecast is identified as "Confirmed" if its type is "New/Confirmed" and its headline contains keywords "affirm", "Affirm", "reiterate", or "Reiterate". A forecast is identified as "New" if its type is "New/Confirmed" and its headline

[1] Our final sample is significantly smaller than that of Radhakrishnan et al. (2012) because we require firm-level data from Compustat Global for all of our analyses, whereas all tests in Radhakrishnan et al. (2012) are conducted at the country level.

does not contain any of the keywords above. A forecast is identified as a revision if its type is "Raised/Lowered".

Below we give two examples to illustrate three different forecast types.

Example 1: A new forecast followed by a forecast revision

On May 29, 2007, Vodafone Group plc (LSE: VOD) provides earnings guidance for the year 2008. For the year, the company expects adjusted operating profit to be in the range of £9.3 billion ($18.4 billion) to £9.8 billion ($19.4 billion).

On November 13, 2007, Vodafone Group plc revised earnings guidance for the full year of fiscal 2008. The company upgraded its revenue guidance to at least £34.5 billion (€49 billion; $71.6 billion) and its operating profit to at least £9.5 billion (€13.5 billion or $19.7 billion).

Example 2: A new forecast followed by a forecast confirmation and then two forecast revisions

On March 26, 2010, Wacker Neuson SE (DB: WAC) provides earnings guidance for the full year of 2010. For the full year of 2010, the company expects revenue to rise by at least 5%. The company also expects an increase in EBITDA and a return to the profit zone at operating level.

On May 28, 2010, Wacker Neuson SE confirmed its 2010 forecast for a revenue increase of at least 5% and a positive operating result. EBITDA is expected to rise on the year.

On August 10, 2010, Wacker Neuson SE revised up earnings guidance for the full year 2010. Based on its first-half performance, the company lifted its 2010 outlook to a revenue growth of at least 10% versus the originally anticipated minimum of 5%. Earnings before interest, tax, depreciation and amortisation (EBITDA) margin is now expected to reach at least 9%, an improvement from the original forecast for a return to the profit zone at operating level.

On November 12, 2010, Wacker Neuson SE revised up earnings guidance for the full year 2010. Based on its performance in the third quarter, the company upgraded its full-year forecast and now expects annual revenue growth of at least 20% and a margin on earnings before interest, tax, depreciation and amortisation (EBITDA) of minimum 10%.

To analyze the impact of IFRS adoption on the different forecast types, we separately count the number of new forecasts *Freq(New)*, forecast revisions *Freq(Revision)*, and forecast confirmations *Freq(Confirm)*. We repeat the analysis in Table 3 by replacing the forecast frequency variable *(Freq)* with the frequency of three respective forecast types. The results are

reported in Table IA2. We find consistent results across all three types, suggesting that IFRS adoption not only encourages managers to issue more forecasts, but also makes them update forecasts more frequently.

Internet Appendix II: Additional Analysis

Analysis of Other Stakeholders

We conduct further analysis on the role of equity markets versus other stakeholders, including government, labor unions, and lenders. We measure the size of a country's equity market as the average equity market capitalization relative to its GDP (*Equity*) for the period 1999-2003 obtained from World Development Indicators at the World Bank on-line database. We measure a country's government involvement using its state ownership (*SOE*), defined as government enterprise and investment at the country-year level. It is scaled from 0 to 10, with higher scores suggesting fewer state-owned enterprises. The data is obtained from Economic Freedom of the World. We multiply this measure by -1, so that a higher value indicates more government involvement. We measure a country's labor union power using the density of its labor unions (*Union*), measured as the percentage of employees as members of a trade union at the country-year level. The data is obtained from the OECD employment on-line database. Lastly, we measure the size of a country's credit market (*Credit*) using domestic credit provided by the banking sector as a percentage of GDP for each country-year. The data is obtained from World Development Indicators at World Bank on-line database.

We re-estimate Equation (1) by allowing the coefficient on *Post_IFRS* to differ between treatment countries with high (above-median) and low values of *Equity*, *SOE*, *Union*, or *Credit*.

The results are reported in Table IA3. We observe that all country groups issue more management forecasts post-IFRS adoption relative to the control group. By comparing coefficients across high and low stakeholder groups, we observe larger increases in management forecasts following IFRS adoption among countries with higher government involvement and smaller equity markets, although this result only holds for regressions on forecasting frequency. This finding provides some limited support to the catching-up effect for firms in countries where the demand for public disclosure was initially low. We do not find the coefficient on *Post_IFRS* to differ when using labor unions and credit market size as partitions, suggesting similar positive effects of IFRS adoption on management forecasts across countries with high and low union and creditor influence.

Robustness Analysis

We conduct a battery of analysis to check the sensitivity of our main results.

Transparency Directive. Another regulatory change within EU that might influence our results is the Transparency Directive, which effectively increased firms' reporting frequency from semiannual reporting to quarterly reporting for many EU countries. Although we have included firms' mandatory reporting frequency (*Interim*) in all our regression analysis, we further explore the impact of the Transparency Directive on our results. Table IA4, Panel A reports the effective date of the Transparency Directive for our sample countries obtained from Christensen et al. (2013b). We re-estimate Equation (1) by adding an additional indicator variable *Post_TPD* defined as one for forecasts issued after the Transparency Directive effective date. The regression results are reported in Table IA4, Panel B, Columns (1) and (2). The

coefficient on *Post_TPD* is positive but only significant for the regression on *Freq*. More importantly, we continue to find the coefficient on *Post_IFRS* to be positive and significant.

Capital IQ coverage. A common concern about databases that provide machine-readable information for management forecasts is their completeness and biases in coverage. For example, by comparing management forecasts provided by Thomson First Call's Company Issued Guidance database with a random sample of management forecasts hand-collected from newswires, Chuk, Matsumoto, and Miller (2013) find that the former tends to cover large firms with high analyst following and institutional ownership and that the coverage is more complete after 1997. Although Capital IQ uses a different data collection process from First Call, it might not be immune to this criticism.[2] In particular, we cannot distinguish when a firm did not issue any management forecasts in a certain year from when the firm was not covered by Capital IQ in that particular year. As a result, our finding that management forecasts increased following IFRS adoption could be driven by Capital IQ's expanded coverage over time. Although our difference-in-difference research design has already taken into account any systematic time trends existing in the data, one may argue that the coverage expansion might be more in favor of the treatment sample, which includes some small economies. If this is the case, having a control group would not entirely control for such a bias. We further address this concern in several ways. First, we limit our sample period to between 2004 and 2010, as Radhakrishnan et al. (2012) suggest that the coverage of Capital IQ is likely to be more complete after 2004. Second, we restrict the analysis to firm-years with at least one forecast, i.e., *Freq>0*, and then examine the impact of IFRS adoption on the number of forecasts. In this way, we limit our sample to firms that are

[2] Capital IQ simply provides all management forecasts in text format and does not code the content of forecasts. Therefore, the bias in First Call that quantitative and bad-news forecasts are more likely to be covered (Chuk et al., 2013) is unlikely to apply to Capital IQ.

already covered by Capital IQ. Third, we restrict the analysis to firms that were forecasters in the pre-adoption period and examine the changes in their forecast behavior after the adoption. The results are reported in Table IA4, Panel B, Columns (3) to (6). We continue to find the coefficient on *Post_IFRS* to be positive and significant across all model specifications. Another potential bias is a higher likelihood to cover English-speaking countries by Capital IQ, although the data provider suggests that they cover both English and non-English information sources. To address this concern, we limit our sample to English-speaking countries only and repeat the analysis. The results as reported in Columns (7) and (8) remain unchanged. The above findings suggest that the observed increase in management forecasts after IFRS adoption is unlikely to be driven by potential coverage biases existing in the Capital IQ database.

Different sample composition and time period. To addresses the concern that our results might be driven by different sample compositions in the pre- and post-adoption periods, we repeat the analysis using a constant sample, where each firm in the treatment sample exists in both pre- and post-adoption periods. These results, as reported in Table IA4, Panel B, Columns (9) and (10), remain unchanged. Our sample period runs from 2002 to 2010 and spans the financial crisis which affected the IFRS adopting countries in the EU severely. If firms affected by the crisis provided more disclosure to reduce the uncertainty faced by investors, we would observe an increase in management forecasts in the post-crisis period. To rule out this alternative explanation, we limit our sample to the pre-crisis period, i.e. before July 2007 or September 2008. Our results, as reported in Table IA4, Columns (11) to (14), remain unchanged.

Country-level analysis. So far, our regressions are conducted at the firm level. To address the concern that our results might be biased towards large economies with large sample

sizes, we repeat our analysis at the country level. Following the methodology in Landsman et al. (2012), we estimate the following equation separately for each country:

$$DISC = \beta_1 Post + Firm\ controls + Industry\ F.\ E. \quad (3)$$

where *Post* is defined as one for fiscal years ending on or after December 31, 2005, and other variables are defined in the same way as before. Since the regression includes a *Post* dummy and is conducted separately for each country, we do not include year or country fixed effects. Table IA5 reports the mean coefficients and Fama and Macbeth (1973) standard errors and *t*-statistics for IFRS and non-IFRS countries separately. A few countries are dropped due to small sample size for country-specific regressions. We observe that 14 out of 21 (10 out of 10) coefficients on *Post* for regressions using firms from IFRS (non-IFRS) countries are positive, suggesting that management forecasts increase in the post-adoption period in both IFRS and non-IFRS countries. The last two columns report the difference in coefficients across these two samples. We find that the difference in the coefficient on *Post* between IFRS and non-IFRS countries is 0.556 and is statistically significant (*t*-statistic=7.628). This corroborates our findings in the firm-level analysis. We obtain qualitatively similar results when we use *Issue* as the dependent variable (unreported).

References (not included in the paper)

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Chuk, E., D. Matsumoto, and G. S. Miller. 2013. Assessing Methods of Identifying Management Forecasts: CIG vs. Researcher Collected. *Journal of Accounting and Economics* 55 (1): 23-42.

Fama, E., and J. MacBeth. 1973. Risk, Return, and Equilibrium: Empirical Tests. *Journal of Political Economy* 81 (3): 607-636.

Table IA1: Distribution of Management Forecasts in the Original Sample

Country	No. of forecasts (2002-2010)	No. of firm-years (2002-2010)	No. of forecasts (2004-2009)	No. of firm-years (2004-2009)	Country	No. of forecasts (2002-2010)	No. of firm-years (2002-2010)	No. of forecasts (2004-2009)	No. of firm-years (2004-2009)
Albania	3	2	3	2	Greece	252	189	233	174
Algeria	4	4	3	3	Greenland	6	3	3	2
Argentina	21	20	14	13	Hong Kong	1298	977	838	640
Armenia	3	3	3	3	Hungary	260	190	213	154
Australia	5579	3025	4399	2439	Iceland	36	24	26	18
Austria	743	379	604	305	India	891	677	720	546
Bahamas	11	8	5	4	Indonesia	528	378	421	302
Bahrain	13	12	10	9	Ireland	723	310	572	242
Bangladesh	4	4	4	4	Israel	817	386	691	320
Barbados	2	2	2	2	Italy	1004	639	852	542
Belarus	2	2	1	1	Jamaica	4	4	3	3
Belgium	511	287	399	234	Japan	41699	15875	39064	14810
Bermuda	319	167	238	124	Jordan	8	7	5	4
Bosnia-Herzegovina	18	15	13	10	Kazakhstan	56	44	44	36
Brazil	248	195	182	153	Kenya	16	14	9	8
British Virgin Islands	13	9	11	7	Kuwait	35	28	25	19
Bulgaria	221	141	200	134	Kyrgyzstan	2	2	2	2
Canada	3620	1934	2517	1325	Latvia	89	76	73	64
Cayman Islands	63	33	50	25	Lebanon	3	3	3	3
Channel Islands	74	41	50	25	Liechtenstein	6	4	5	3
Chile	55	49	47	41	Lithuania	435	218	405	198
China	4189	2214	3019	1655	Luxembourg	186	92	137	71
Colombia	33	31	28	27	Macau	4	3	2	1
Croatia	64	54	45	40	Macedonia	16	16	7	7
Cyprus	628	423	475	327	Malaysia	944	777	626	531
Czech Republic	330	250	303	229	Malta	6	5	4	3
Denmark	2150	889	1700	740	Mauritius	3	3	2	2
Ecuador	3	3	3	3	Mexico	152	91	110	70
Egypt	16	15	8	8	Moldova	14	12	10	9
Estonia	53	47	49	43	Monaco	6	5	4	3
Fiji	2	2	2	2	Mongolia	2	2	2	2
Finland	1448	642	1073	518	Montenegro	2	2	1	1
France	2372	1333	1824	1068	Morocco	10	8	7	5
Georgia	3	3	2	2	Namibia	3	2	3	2
Germany	5616	2442	4471	2013	Netherlands	1105	578	877	468

Gibraltar	8	5	4	3	Netherlands Antilles	33	13	24	9
New Zealand	649	384	511	308	South Korea	404	297	345	260
Nigeria	59	54	28	25	Spain	529	385	439	320
Norway	307	224	211	160	Sri Lanka	16	14	13	11
Oman	11	11	8	8	Sweden	604	379	479	308
Pakistan	41	39	31	29	Switzerland	1245	700	1018	581
Panama	9	8	6	5	Taiwan	491	300	410	251
Papua New Guinea	12	11	8	8	Tanzania	6	6	2	2
Peru	14	13	12	11	Thailand	1451	894	1268	768
Philippines	459	291	365	236	Trinidad & Tobago	3	2	1	1
Poland	651	404	618	383	Tunisia	5	5	4	4
Portugal	139	109	101	85	Turkey	45	39	32	29
Qatar	17	15	12	12	Uganda	3	3	2	2
Romania	672	454	615	404	Ukraine	106	86	81	65
Russia	1189	697	939	562	United Arab Emirates	93	80	69	61
Rwanda	6	3			United Kingdom	3486	2500	2637	1945
Saudi Arabia	17	15	14	12	United States	65037	22389	49290	16044
Serbia	51	40	35	28	Uruguay	6	6	6	6
Singapore	733	496	501	341	Venezuela	15	12	11	9
Slovakia	53	52	51	50	Vietnam	1780	888	659	391
Slovenia	166	108	140	92	Zimbabwe	7	6	2	1
South Africa	528	421	421	345	**Total**	**34,225**	**18,534**	**26,388**	**14,645**

Table IA2: Analysis of Management Forecast Types

This table repeats the analysis in Table 3 by using different forecast types. *Feq(New)* measures the number of new forecasts, *Freq(Revision)* measures the number of forecast revisions, and *Freq(Confirm)* measures the number of forecast confirmations. We use a Poisson model for all regressions. We report coefficient estimates and standard errors (in parentheses) based on robust standard errors clustered by country. All regressions include country, year (calendar year of forecast date), and industry (2-digit SIC) fixed effects. All continuous variables are winsorized at the 1st and 99th percentiles. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

	Benchmark: All Non-IFRS Adopters			**Benchmark: PSM Non-IFRS Adopters**				**Benchmark: Voluntary Adopters**		
	(1)	(2)	(3)	(4)	(5)	(6)		(7)	(8)	(9)
	Freq (New)	*Freq (Revision)*	*Freq (Confirm)*	*Freq (New)*	*Freq (Revision)*	*Freq (Confirm)*		*Freq (New)*	*Freq (Revision)*	*Freq (Confirm)*
Post_IFRS	**0.982*** **	**1.369*** **	**1.593*** **	**0.868*** **	**1.203*** **	**1.298*** **	***Post_Mandatory***	**0.190*** **	**0.203*** **	**0.171** **
	(0.221)	**(0.399)**	**(0.443)**	**(0.158)**	**(0.273)**	**(0.414)**		**(0.055)**	**(0.072)**	**(0.080)**
							Mandatory	-0.267***	-0.241***	-0.277***
								(0.052)	(0.093)	(0.085)
Size	0.142***	0.166***	0.139***	0.157***	0.181***	0.163***	*Size*	0.144***	0.197***	0.242***
	(0.013)	(0.028)	(0.041)	(0.011)	(0.050)	(0.046)		(0.015)	(0.022)	(0.030)
ROA	0.488***	0.897***	0.618***	0.330***	0.677***	0.703**	*ROA*	0.242	0.606***	-0.015
	(0.085)	(0.286)	(0.199)	(0.071)	(0.108)	(0.302)		(0.156)	(0.123)	(0.270)
BTM	0.002***	0.003***	0.001	0.000	0.002	0.001	*BTM*	-0.000	-0.016	-0.109*
	(0.000)	(0.000)	(0.001)	(0.001)	(0.003)	(0.002)		(0.001)	(0.020)	(0.062)
Leverage	0.098	0.522***	0.395***	-0.018	0.454***	0.255***	*Leverage*	0.185***	0.427***	0.222*
	(0.067)	(0.094)	(0.103)	(0.069)	(0.077)	(0.097)		(0.063)	(0.113)	(0.127)
EarnVol	-0.006***	-0.025	-0.037	-0.016	-0.046	-0.023	*EarnVol*	0.000	-0.101	-0.106
	(0.001)	(0.018)	(0.036)	(0.016)	(0.030)	(0.016)		(0.009)	(0.087)	(0.115)
RetVol	0.049	0.020	-0.008	0.063*	0.032	0.077*	*RetVol*	0.099**	0.215***	0.134***
	(0.040)	(0.043)	(0.032)	(0.035)	(0.043)	(0.045)		(0.041)	(0.046)	(0.037)
Analysts	0.011***	0.014	0.008	0.006	0.014	0.006	*Analysts*	0.008**	0.009**	0.001
	(0.003)	(0.009)	(0.009)	(0.005)	(0.011)	(0.011)		(0.004)	(0.004)	(0.004)
Inst	0.288**	0.332**	0.280**	0.267***	0.241*	0.078	*Inst*	0.175**	0.604***	0.543***
	(0.118)	(0.139)	(0.110)	(0.074)	(0.131)	(0.184)		(0.072)	(0.206)	(0.195)

BadNews	-0.072***	-0.084	-0.162***	-0.071***	-0.090	-0.167***
	(0.021)	(0.099)	(0.030)	(0.016)	(0.082)	(0.025)
EquityIssue	-0.033	0.034	-0.064	-0.053*	0.010	0.053
	(0.027)	(0.074)	(0.066)	(0.029)	(0.060)	(0.141)
ADR	0.205**	0.142	0.271***	0.197**	0.204*	0.393***
	(0.097)	(0.120)	(0.089)	(0.084)	(0.123)	(0.132)
Interim	0.247***	0.113**	-0.027	0.225***	0.083*	-0.097**
	(0.075)	(0.051)	(0.044)	(0.060)	(0.048)	(0.040)
Year, Country, and Industry Fixed Effects Included						
Observations	80,877	80,877	80,877	40,584	40,584	40,584
Pseudo Rsq	0.208	0.225	0.216	0.227	0.267	0.236

BadNews	-0.028*	-0.041	-0.111**
	(0.016)	(0.095)	(0.053)
EquityIssue	0.001	0.053	-0.167
	(0.029)	(0.068)	(0.107)
ADR	0.122**	0.113	0.198***
	(0.056)	(0.116)	(0.075)
Interim	0.096	0.074	0.163
	(0.071)	(0.099)	
	χ^2-test for Post_Mandatory+Mandatory=0 [p-value]		
Post_Mandatory +Mandatory	[0.02]	[0.00]	[0.01]
Year, Country, and Industry Fixed Effects Included			
Pseudo Rsq	24356	24356	24356
Observations	0.219	0.223	0.255

Table IA3: Analysis of Stakeholder Influence

This table repeats the analysis in Table 4, Columns (1) and (2) by replacing legal origins with stakeholder influence. *SOE* includes government enterprise and investment measured at the country-year level. It is scaled from 0 to 10, with higher scores suggesting fewer state-owned enterprises or lower government influence. The data is obtained from Economic Freedom of the World. We multiply the measure by -1 so that higher values indicate higher government influence. *Equity* is equity market capitalization of a country relative to its GDP for the period 1999-2003 obtained from World Development Indicators at the World Bank on-line database. *Union* is density of labor union and is measured as the percentage of employees who are members of a trade union at the country-year level. The data is obtained from OECD employment on-line database. *Credit* is domestic credit provided by banking sector as a percentage of GDP for each country-year. The data is obtained from World Development Indicators at the World Bank on-line database. $Post\_IFRS_{High}$ ($Post\_IFRS_{Low}$) is defined as one for observations from the IFRS countries with high (low) stakeholder influence and with fiscal year ends on or after mandatory adoption date, i.e. December 31, 2005, and zero otherwise. Countries with above-median values of *SOE*, *Equity/GDP*, *Union*, and *Credit* are classified as having high stakeholder influence. Sample median is calculated using IFRS countries only. We use a Logit model for regressions on the binary variable, *Issue*, and a Poisson model for regressions on count variables *Freq*. We report coefficient estimates and standard errors (in parentheses) based on robust standard errors clustered by country. All continuous variables are winsorized at the 1st and 99th percentiles. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

		SOE		**Equity**		**Union**		**Credit**	
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pred. Sign	*Issue*	*Freq*	*Issue*	*Freq*	*Issue*	*Freq*	*Issue*	*Freq*
$Post\_IFRS_{High}$	+	1.869**	1.444***	1.215**	1.057***	1.534**	1.254***	1.579**	1.190***
		(0.821)	(0.392)	(0.581)	(0.329)	(0.644)	(0.349)	(0.757)	(0.364)
$Post\_IFRS_{Low}$	+	1.391**	1.133***	1.822***	1.314***	1.880**	1.245***	1.586***	1.301***
		(0.570)	(0.317)	(0.651)	(0.355)	(0.748)	(0.357)	(0.465)	(0.283)
				χ^2-test for difference [p-value]:					
$Post\_IFRS_{High}$= $Post\_IFRS_{Low}$		[0.19]	[0.01]	[0.00]	[0.18]	[0.15]	[0.88]	[0.99]	[0.51]
		Firm-level Controls Included Year, Country, and Industry Fixed Effects Included							
Observations		0.221	0.242	0.221	0.242	0.215	0.223	0.225	0.243
Pseudo Rsq		80,839	80,839	80,877	80,877	72,016	72,016	79,087	79,087

Table IA4: Robustness Analysis

Panel A reports the Transparency Directive date for IFRS countries. *TPD Date* is the effective date (year and month) of the Transparency Directive and is obtained from Christensen et al. (2013b). Panel B reports multivariate regression results for difference-in-difference analysis using all non-IFRS adopters as the control group. In Columns (1) and (2), *Post_TPD* is defined as one for observations from the IFRS countries and if the forecast was issued after the Transparency Directive date, and zero otherwise. It is defined as zero for observations from IFRS countries without Transparency Directive date. In Columns (3) and (4), we keep only forecasts issued between 2004 and 2010. In Column (5), only forecasters, i.e. firm-years with at least one forecast, are used in the regression. In Column (6), only firms that forecasted in the pre-adoption period are used in the regression. In Columns (7) and (8), we keep only English-speaking countries in our sample, including Australia, Hong Kong, Ireland, Philippines, the UK, the US, India, and Canada. In Columns (9) and (10), we keep only a constant sample of firms, i.e. firms existing in both pre- and post-adoption periods. In Columns (11) and (12), we keep only forecasts issued before July 2007, and in Columns (13) and (14), we keep only forecasts issued before September 2008. In Columns (15) and (16), we exclude all the fixed effects. We use a Logit model for regressions on the binary variable, *Issue*, and a Poisson model for regressions on the count variable, *Freq*. We report coefficient estimates and standard errors (in parentheses) based on robust standard errors clustered by country. All regressions include firm-level controls (as defined in Table 3), country, year (calendar year of forecast date), and industry (2-digit SIC) fixed effects. All continuous variables are winsorized at the 1st and 99th percentiles. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

Panel A: Transparency Directive date for IFRS countries

Country	TPD date
Australia	
Austria	200704
Belgium	200809
Czech Republic	200908
Denmark	200706
Germany	200701
Finland	200702
France	200712
Greece	200707
Hong Kong	
Hungary	200712
Iceland	200711
Ireland	200707
Italy	200904
Luxembourg	200801
Netherlands	200901
Norway	200801
Philippines	
Poland	200903
Portugal	200711
South Africa	
Spain	200712
Sweden	200707
Switzerland	
United Kingdom	200701
Venezuela	

Panel B: Regression results

	Transparency Directive		2004-2010		Only Forecasters	Only Pre-adoption Forecasters	English-speaking countries	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Freq	*Issue*	*Freq*	*Issue*	*Freq*	*Freq*	*Freq*	*Issue*
Post_IFRS	1.110***	1.302**	1.109***	1.474*	0.426**	0.657**	0.603***	0.777***
	(0.299)	(0.512)	(0.429)	(0.790)	(0.170)	(0.315)	(0.053)	(0.126)
Post_TPD	0.309**	0.669						
	(0.140)	(0.485)						
Observations	80,877	80,877	64,652	64,652	36,963	45680	40,221	40,221
Pseudo Rsq	0.242	0.222	0.198	0.165	0.086	0.213	0.191	0.236

	Constant Sample		Before July 2007		Before September 2008		No Fixed Effects	
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
VARIABLES	*Freq*	*Issue*	*Freq*	*Issue*	*Freq*	*Issue*	*Freq*	*Issue*
Post_IFRS	1.243***	1.515**	1.070***	0.931***	1.103***	1.121***	1.353***	1.440***
	(0.316)	(0.617)	(0.237)	(0.208)	(0.287)	(0.388)	(0.413)	(0.365)
Observations	68,537	68,537	46,257	46,257	55,732	55,732	80,876	80,876
Pseudo Rsq	0.218	0.24	0.280	0.288	0.264	0.261	0.117	0.091

Table IA5: Country-level Analysis

This table presents country-level Fama and MacBeth (1973) regression coefficients and statistics using Poisson regressions. *Post* is a dummy variable equal to one for fiscal years ending in or after December 2005. Control variables are defined in Table 2. All regressions include industry fixed effects. All continuous variables are winsorized at the 1st and 99th percentiles.

	Non-IFRS Countries			**IFRS Countries**			**IFRS - Non-IFRS**	
	Dependent Variable = *Freq*			Dependent Variable = *Freq*				
	Coef.	*Std Err*	*t-stat*	*Coef.*	*Std Err*	*t-stat*	*Diff*	*t-stat*
Post	**0.466**	**0.161**	**2.898**	**1.022**	**0.239**	**4.278**	**0.556**	**7.628**
Size	0.196	0.081	2.426	0.124	0.089	1.394	-0.072	-2.261
ROA	0.441	0.914	0.483	-0.036	0.696	-0.052	-0.477	-1.461
BTM	-0.010	0.081	-0.124	-0.003	0.131	-0.025	0.007	0.177
Leverage	-0.283	0.386	-0.732	0.117	0.519	0.225	0.400	2.399
EarnVol	-0.035	0.850	-0.041	-0.216	0.960	-0.225	-0.181	-0.530
RetVol	0.172	0.150	1.152	0.216	0.167	1.296	0.044	0.739
Analysts	-0.006	0.015	-0.414	0.013	0.020	0.662	0.020	3.001
Inst	0.223	0.582	0.384	0.229	0.436	0.526	0.006	0.028
BadNews	-0.143	0.126	-1.131	-0.034	0.147	-0.231	0.109	2.129
EquityIssue	-0.014	0.305	-0.047	-0.115	0.260	-0.443	-0.101	-0.900
ADR	0.337	0.278	1.214	0.021	0.286	0.072	-0.316	-2.939
Interim	0.205	0.270	0.757	0.703	0.296	2.372	0.498	4.643
N		10			21			
N Positive		10			14			
Pseudo Rsq		0.098			0.159			